THE

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OF

THE ASIATIC SOCIETY

OF

BENGAL.

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THE JOURNAL OF THE ASIATIC SOCIETY OF BENGAL.

EDITED BY

JAMES PRINSEP, F. R. S.

SECRETARY OF THE ASIATIC SOCIETY.

VOL. II.

JANUARY TO DECEMBER,

1833.

"It wiil flourish, if naturalists, chemists, antiquaries, philologers, and men of science, in different parts of *Asia*, will commit their observations to writing, and send them to the Asiatic Society at Calcutta; it will languish, if such communications shall be long intermitted; and it will die away, if they shall entirely cease."

Sir Wm. Jones.

Calcutta:

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1833.

PREFACE.

On completion of this second volume of the Journal of the Asiatic Society, the Editor feels it to be due to his subscribers, as well as to himself, to lay before them as briefly as possible, the results of the arrangements which he contemplated carrying into effect at the conclusion of the last volume;—more especially as a somewhat erroneous estimate of the cost and circulation of the Journal found admission into a late notice of the Indian Periodical Press, drawn up by the Editor of one of the morning papers. The Journal is not published, as there stated, by the Asiatic Society, but solely at the cost and responsibility of the Secretary, who was Editor of it before he enjoyed the honour of an election to that office. Since there never has been the least view to profit, either in the Gleanings or in the present work, there can be no object whatever in concealing any information respecting its publication; and it may be useful hereafter to find on record a note of the expences of printing, and the difficulties against which a Journal exclusively scientific has had to contend, as well as the advantages which it has enjoyed, in India at the present time. The following particulars have therefore been extracted from the accounts of the two years now terminated.

The amount of subscriptions to the Journal at one rupee per number, including two extra numbers, in 1832, was .... Rs.		5148 8
From this, deducting 20 per cent. commission paid to Messrs. Thacker and Co. for circulating it, ...		1028 11
There remained net subscriptions available, Rs.		4114 13
The Baptist Mission Press charged for printing and stitching 500 copies,	Rs. 3742 10	
And the 15 plates cost with printing,	416 5	
Total		4178 5

The result of the first year exhibits a sufficient accordance between outlay and return. Of the amount subscribed however, only Rs. 3786 13 have been collected up to the present time, so that in fact there was a deficit of Rs. 392 2.

The alterations which the Editor proposed and completed for the second year were :—

1. The saving of nearly half of the commission paid for the mere circulation of the work (without responsibility), by undertaking that duty with the aid of his establishment as Secretary of the Asiatic Society;

2. As a return for this favor, he proposed circulating the Journal gratis to such of the paying members as should express a desire to take it in.

The effect of this scheme has been as follows :

Fifty members of the Society have availed themselves of the privilege, which has made a deduction to the same amount from the monthly receipts. The number of copies circulated, including those sent to subscribers and societies in Europe, is about 450.

The number of paying subscribers on the list, is 320, which at 1 R. per month, (including one extra number of Buchanan,) would give Rs. 4480.

The expenses of printing 500 copies, of 670 pages,

at 4-5 per page, may be stated at	Rs. 2,890	
144 pages of Buchanan, at 4-8 per page,........	648	
Covers, table work, &c. charged extra, ...	250	
40 pages of Appendix, at 5 Rs..................	200	
28 plates (18 lithographs, 10 engravings*), ...	480	
Establishment for circulation,.................	600	
	——	5,068

Leaving a loss on the year of Rs. 588, or nearly as much as the subscriptions of the members exempted from paying.

But it must be mentioned, and mentioned with a degree of disappointment which is almost disheartening, that of the flattering list of sub-

* For these the cost of printing and paper only is charged.

scribers above given, 70 have not paid any part of the year's subscription, and as many more are still in arrears; so that a balance of Rs. 1321-8 still remains to be collected. The actual state of the concern is therefore by no means so favorable as could be wished, for it leaves the Editor out of pocket upwards of 2000 Rs. as the reward of his labour for two years ! B əq ʇnwill not for a moment suppose that the balances outstanding are not recoverable : on the contrary the principal difficulty lies in the distance, and the supposed want of a mode of remittance.—Many subscribers are not aware, that letters containing hoondees for the amount may be transmitted *post free* to the Editor.

It will be remembered, that the Bengal Government were pleased to bestow the privilege of free postage on the GLEANINGS and on the JOURNAL, on condition of the publication of the late Dr. Buchanan's Statistical Reports. Under the impression (justly formed) of a corresponding increase of circulation, consequent upon this liberal boon, it was resolved not to incorporate these records in detached notices in the JOURNAL, nor to diminish from its original matter*, but to publish them as a separate work ; and one volume has accordingly been completed, containing 356 pages, which at 4-8 per page have cost Rs. 1,602

And a reprint of the first 108 pages, which became necessary on the subsequent extension of the edition from 300 to 500 copies, 216

Total, Rs. 1818

This expence has been incurred therefore on account of Government, in return for the postage saved, not to the work, but to the subscribers of the JOURNAL. On the completion of the first volume of BUCHANAN, a second extra volume of an official nature on the Monetary System was commenced, of which 50 pages have been printed with 3 plates, being in fact an expence of more than 300 rupees not included in the above estimate. The Government meantime placed the remaining volumes of Buchanan in the Editor's hands, with an intimation of its "desire that the printing of these records should be continued." It was therefore with no small feeling of mortification that

* Originally 32 pages only were given in each number, latterly 64.

the EDITOR perused the following letter, announcing that the privilege of free postage should cease from June next, especially after having been honored, on an explanation of the nature of the work, with an extension of the same privilege to the Madras presidency, in addition to that formerly bestowed by the Governors of Bombay and Ceylon.

To JAMES PRINSEP, Esq.

Genl. Dept. Editor of the Journal of the Asiatic Society,

Sir,

I am directed to inform you, that the Governor General in Council has resolved, that after six months the exemption from postage, which is now enjoyed by the Journal of the Asiatic Society, shall be discontinued.

I have the honor to be,
Sir,
Your most obedient servant,
G. A. BUSHBY,
Offg. Sec. to Govt.

Council Chamber,
2nd Dec. 1833.

It may reasonably be feared that many subscribers at distant stations may be unable to continue their support to the work, when its cost shall be enhanced by postage; but (should it be impossible, on a proper and respectful representation of the circumstances, to avert the imposition of postage) every means will be taken of lessening the burthen by sending the monthly numbers by the bangy instead of the regular dâk.

On the contents of a volume which has already been perused by nearly all to whom it circulates, it would have been obviously needless to make any remark, were it not desirable to prove that the favors hitherto conferred upon the work by the Government of the country had not been altogether misapplied.

Independently of the volume of Dinajpur Statistics, which forms a model for the use of public officers engaged in collecting similar information, the GLEANINGS and the JOURNAL have been the means of bringing to notice many of the mineral resources of our vast Indian Empire, and of leading to fresh discoveries by the announcement of what had already been found: coal may be adduced as an example,—of which twenty or more different localities have been brought to our knowledge through its pages, where only two were before known. Of the native mineral productions, iron, copper, gold, &c.:—Of the native arts and manufactures, salt, nitre, turpentine, dyes, mills, &c. numerous original ac-

counts have been inserted : catalogues of woods, medicinal plants and drugs : experiments on materials, wood, iron, cement ;—Statistical reports ;—descriptions of newly explored countries and people :—in fact, it would be difficult to open a number of the JOURNAL without finding some information which must possess value in the eyes of a government. Contributions of a more exclusively scientific nature have, in the mean time, continued to multiply, and the objects pointed out as desiderata at home in the geography, meteorology, geology, and natural history of this country, are in the course of rapid and systematic elucidation. So numerous for instance have been the registers of the weather offered for publication, that space could only be found for abstracts of many. There has hardly been time for the collection of materials regarding the tides of the Indian coasts, suggested in the Rev. Professor WHEWELL's circular, (inserted in page 151,) but the attention of those who have opportunities of eliciting the information required, is again solicited to this object.

As a proof of the benefit conferred on science by the free and extensive circulation of a periodical devoted to such objects, the Editor feels pride in alluding to the ardour which his plates of ancient coins have inspired in many active collectors, and above all to the reward bestowed ou himself by the munificence of General VENTURA, the most successful pursuer of antiquarian research in the Panjáb, who has presented to him all the coins and relics discovered on opening the celebrated Tope of Manikyala. They are now on their way to Calcutta.

That extracts and analyses of European science have not been more frequent must be attributed once more to want of space and want of leisure. The Editor would recommend all who seek for knowledge of the progress of science in Europe to procure a copy of the Reports of the British Association for 1832, in which they will find every branch discussed by the philosopher best able to give it illustration. To attempt to shorten those admirable essays would be mutilation rather than abridgment ; yet unfortunately most of them are too long for the pages of a monthly journal.

On the subject of orthography of native words, the Editor is driven to make one concession, for which he fears the learned Societies at home

will denounce him as an apostate to the system of their leader. Every communication, with hardly any exception, which comes for publication, adopts the Gilchristian mode of spelling, or that modification of it which has been *ordered* to be used in all Government records, surveys, &c. An attempt has been made hitherto to conform the whole to Sir William Jones' method, but necessarily there have been continual omissions, and the contributors in most cases express themselves but ill pleased to see their words transformed into shapes but ill accordant with ordinary *English* pronunciation. The Editor has therefore resolved to adopt the middle course followed in Hamilton's Hindustan, namely, to print all Indian names and words in the ordinary roman type as they are usually written and pronounced, and to place in italics all such native terms and proper names, as are corrected, and spelt according to the classical standard of Sir William Jones: in many cases the latter may be inserted in brackets after the ordinary word.

Where contributors have occasion to illustrate their papers by plates, it will be a great convenience to the Editor to have the original drawings prepared of the same dimensions as the printed page of letter press, to save the trouble and expence of reducing them.

The Editor will not allude in this place to the severe loss he has sustained in the death of some of the most able and constant supporters of his work, and the departure to Europe of others in the course of the past year; since he hopes that a more worthy channel will be found for the record of their meritorious labours for the cause of Science in India, in the Proceedings of the Asiatic Society, to which their names belong, and in which their reputation must ever be cherished with fond remembrance.

1*st January*, 1834.

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I.—*An Inquiry into the Laws governing the two great powers, Attraction and Repulsion, as operating on the Aggregation and Combination of Atoms. By* Julius Jeffreys, Esq.

Though the causes of the three states of matter, as they are called, that is to say, the solid, the liquid, and the aeriform, together with those causes by which the union of the different kinds of matter in compound bodies is effected, and those also by which bodies are expanded, contracted, or preserved of the same magnitude are subjects of great curiosity and importance, yet they belong to a branch of Chemistry which is at present in an unadvanced and imperfect state. Those justly celebrated philosophers who have done honor to our age by their discoveries in other branches have not yet carried their examination so far into this part as to arrive at any settled opinions concerning it, and not unfrequently in the same author doctrines have been advanced which are irreconcilable with each other.

The branch of natural philosophy to which the present inquiry is devoted having continued, with little advance, since it was written, in the year 1822, the doctrines I have endeavoured to establish, and the body of arguments by which they are supported, maintain still whatever of novelty or importance they may have possessed. As, however, in so considerable a period, a few of the arguments may have been brought forward by others, though not perhaps similarly applied, I have thought it proper to mark by including brackets, thus [ ], such parts as have undergone any alteration upon a revision. The body of the work remains *verbatim* as when first written.

Whether by directing my attention to this part of Chemistry I have been enabled to suggest any such modes of reasoning as may be applied

to the advantage of the science, it would be presumption in me to pronounce an opinion. The question must depend upon the strength of the arguments which I use, and which I now submit to the judgment of the philosophic public.

Part I.—*Of Attraction.*

Attraction is usually divided into two kinds.

The first of them Gravitation, or that by which bodies have a tendency to approach each other, and on which the sciences of Mechanics and Physical Astronomy depend. The second Contiguous Attraction, or Attraction of Atoms, by which the atoms of bodies are kept in connection with each other, and which alone it is my province at this time to consider.

Contiguous Attraction, by a division subordinate to the former, is usually considered as comprehending two species, *Attraction of Aggregation*, or the attraction existing between homogeneous atoms, and *Chemical Attraction*, or that which is between heterogeneous atoms.

This distinction has arisen from a supposition, that similar particles exert an attraction towards each other which obeys laws different from those of the attraction between dissimilar particles. That such is an unnecessary distinction might be inferred, were there no other, from this consideration, that when one solid combines with another to form a compound solid, it is not possible to make a distinction between the attraction uniting its compound particles and the affinity by which the constituents are united. Thus in sulphuret of iron the cohesion of the iron and the sulphur is overcome by their mutual attraction which forms them into compound particles, and these again cohere in a new solid differing entirely from either of the former. The attraction which keeps the particles of the sulphuret in a state of aggregation cannot be distinguished from that which brought their elements together; for it favors the union of the elements, and aids in preventing their separation.

With reference to this and other differing opinions relative to contiguous attraction, I propose to begin this Essay by a somewhat minute examination of it under the following heads. 1st. By inquiring into the distance at which it operates; whether it is confined to near particles only, or extends to more remote ones. 2ndly. By inquiring how far the attraction of atoms is general; that is, whether all atoms in nature attract, and are attracted by all, or whether attraction between atoms (chemical and cohesive) is confined to a limited number. 3rdly. By inquiring into the effect of mass on contiguous attraction;

that is, how far the attraction between atoms (chemical and cohesive) is increased towards any given atom by the mass.

4thly. In what ratio of the distance the force of attraction of atoms varies; which will lead me to confirm by arguments the opinion that gravitation and contiguous attraction are the same property of matter, differing only in the circumstances under which it is presented to our observation.

1*st. Of the distance at which attraction of atoms operates.*

From the effects of cohesive attraction being in most cases evident only at very small distances, and from the particles of bodies in the aerial state actually appearing to repel each other, it has been generally inferred that this force is exerted only at very limited distances; and hence its name, contiguous attraction.

Although the effects of cohesive attraction may be *apparent* only at very small distances, yet it is scarcely correct to infer that this force is *exerted* only at such distances until due attention has been paid to the causes, which, by affecting the phenomena, may create deception upon the mind. These appear to me of two kinds,—the minuteness of attracting atoms, and all causes which operate *against* the attraction of atoms.

On the magnitude of atoms must in a great measure depend the greatest distance at which the force of their attraction is sufficiently powerful to be apparent.

If it be admitted that the force of this attraction decreases in as great a ratio of the distance as that of gravitation, then, since atoms are so small as not to be perceptible to our senses, it will follow that however strong their attraction may be when almost contiguous to each other, it will not be apparent at any mensurable distance, though in fact, it may be exerted in some degree at unlimited distances; for if two attracting particles of matter were sufficiently increased in magnitude without altering at all the laws of their attraction, this force might be evident at any distance however great, unless it be supposed (which would indeed be very unphilosophical) that attraction ceases at some certain distance suddenly and abruptly.

The other causes which may create deception as to the distance at which this attraction is exerted, are all powers which oppose its force. From the attraction of particles being constantly opposed by the powerful agency of heat, its force in liquids is scarcely apparent, though in fact it may be very powerful, for it is only the excess of the attraction over the repulsion that can be measured.

The two following are proofs of attraction in liquids, and also that it is very considerable*.

Sir HUMPHREY DAVY remarks very justly, "Cohesion is usually said to act only at the surface of bodies, or by their immediate contact, but this does not seem to be the case. It certainly acts with much greater energy at small distances; but the spherical form of minute portions of fluid matter can only be produced by the attraction of all the parts of which they are composed for each other; and most of these attractions must be exerted at sensible distances." To this remark, I may be allowed to add, that the attraction between the particles of a liquid, must, moreover, be a very powerful force; for it is not only able to resist the force of repulsion, but also to gather the particles into drops against their tendency to gravitate.

Another proof of the force of attraction in liquids, appears to me afforded in the fact, that the expansion of liquids increases in a greater ratio than the temperature, or that liquids expand more from equal additions of heat at high than at low temperatures. If the pressure of the atmosphere were the only force opposing their expansion, liquids would expand less as the temperature increased; for, as a liquid expands, since it presents a greater surface† either to the air or to the vessel containing it, it is pressed on with increasing force. But if the force opposing the expansion increases with the temperature, it is plain that equal additions of heat would produce less and less effect.

If these equal increments of temperature in liquids be considered to indicate equal additions of heat, as is the general opinion, the only means by which the increased ratio of expansion can be accounted for, it appears to me, must be sought for in a powerful attraction exerted between the particles of a fluid, by the decrease of which attraction, as the particles separate from each other, more effect is produced towards enabling heat to expand the fluid, than the increase of atmospheric pressure produces in *opposing* the expansion; so that the sum of the powers *opposing* expansion is a decreasing force, and hence the expansion itself will have an increasing ratio.

The nature and physical properties of gases, have especially induced most philosophers to consider the attraction of atoms as only acting

* Elements of Chemical Philosophy, p. 68.

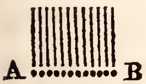
† This is of course the same effect; for though a liquid expanding in a jar may not have the surface exposed to the air increased, yet it displaces more and more air, and is re-acted on by the vessel, with precisely the same force, as if it were compressed on all sides by the atmosphere.

when atoms are very near to each other. From the great elasticity of gases, their atoms are treated of as beyond the sphere of mutual attraction, and some philosophers* have accounted for the expansion being equable for each equal addition of temperature, and for the ratio of expansion being the same in all gases, by the supposition of *no* attraction existing between their particles, and as proofs of the non-existence of any such attraction. How far this reasoning is correct let us presently examine.

The elasticity of a gas is certainly no proof of the absence of any attraction between its atoms. It serves only to show that the whole repulsion is very powerful and superior to any attraction that may exist between its atoms. Since the attraction between the particles of a gas is inferior to the repulsive power, it cannot be apparent, though it may yet certainly exist.

The gas will possess elasticity, and will expand unless subjected to a compressing force, such as the atmosphere.

Again, the equable expansion of a gas from each equal rise of temperature, is not any proof of the absence of attraction between its atoms on the ground usually taken, that if there were any attraction present, it ought, by decreasing as the atoms separated, and consequently offering less and less resistance, to allow of an increasing expansive effect from each equal addition of temperature. For it will presently be seen, that equal increments of temperature in gases by no means indicate equal quantities of heat, and therefore not equal additions of repulsive power. Neither does the atmospheric pressure offer constantly equal resistance to the expansion of a gas; since as a gas expands this force tending to compress its atoms must increase, for as the particles of a gas recede from each other, each is subjected to and



has to support the pressure of a greater number of those of the atmosphere. Thus in the annexed figure the line A B may be supposed to mark the contact of the atmosphere, and a volume of subjacent gas. Here, each particle of gas with its elastic medium†, denoted by the dots A B, is subjected to the pressure of a column whose base is one particle of air.

Let the gas be expanded by heat until the distance of the particles from each other is double. It is now clear, that each particle with its elastic medium (now greatly

* Murray, vol. i. p. 248, System of Chemistry. Berthollet, Chem. Statics. Lambert, vol. i. p. 116, 117, and 143.

† The seat of the repulsive force, according to most authors; as will be presently considered.

enlarged) has to support a column of air, the base of which is four particles, two being shown in the side view. Hence the atmospheric force tending to compress any two gaseous particles must increase as they recede from each other; and even very considerably, for aerial fluids expand much from small increments of temperature*.

The experiments of Mr. Dalton, De Luc, and others, made chiefly between the freezing and boiling points of water, lead to the conclusion that gases expand $\frac{1}{480}$ (of their bulk at 32°) with each accession of temperature of one degree (Farh.) in a simple arithmetical progression; and it appears assumed that this is the law of their expansion by heat. Hence air at 32° by an advance of 480 degrees, i. e. to 512°, would have its bulk doubled. Let us suppose two cubical pints of air to be taken; and let one of them be expanded to double its bulk, i. c. to a quart. Since the distance of the atoms increases as the cube root of the bulk; the bulk of one of these portions of air having become 2 to the other as 1; the distance of the atoms will have increased in the former in the ratio of the cube root of 2 to the cube root of 1, i. e. as 1.26 to 1 nearly; and since the number of atoms under a given surface of the gas expanded to a quart will be 100, while there are 158 under the same surface in the pint, and the pressure being constant on a given surface, 100 atoms of the former will have to support as much as 158 of the latter. Let the pressure be called 158. It is plain each particle of the quart will be pressed on by a force 1.58, while each of the pint will have to bear only a pressure of 1.

Again, since, as was shown by Newton, the mutual elasticity of the particles of air (and the same is assumed with regard to all gases), varies inversely as their distance, i. e. decreases in the direct proportion of their separation; and since the pressure increases as the square of their distance; the total absolute force expanding a gas must be in-

* The reader will not, it is hoped, think that the following error is here committed of supposing that by increasing the surface of a volume of gas the compression of its parts is increased; as for instance, that the compression of the parts of a spherical pint of gas (in which form the surface is the least possible) would be increased by moulding the volume into any other form, as that of a long cylinder, where the surface would be greatly increased. So long as the number of particles in a given volume is constant, the pressure and mutual re-action of the atoms will of course not vary, whatever may be the extent of surface exposed to the atmosphere or to any vessel it is contained in. But directly the number of particles in a given bulk, ceases to be constant owing to expansion, the pressure on each particle, of necessity must increase, whether it be a superficial particle contiguous to the air, or inside of the vessel, or a central one receiving the pressure from the other particles and re-acting against it.

creased in the direct ratio of the increase of the bulk.—Thus one cubic inch of air will need the absolute elastic power of each particle to be increased eight times, in order to expand it to 8 inches. The bulk having been increased eight-fold, the distance of the particles will be doubled (i. e. as the cube root of the bulk); on doubling their distance their elastic force is halved, i. e. from 8 it has become 4, and at the same time the pressure is increased as the square of their distance 2, and is therefore 4. Here then the elasticity and pressure balance each other, and the particles will be stationary. Hence the power endowing the particles with mutual elasticity must have been increased in the same ratio as the increase of the bulk. If these 480 degrees of temperature can double the bulk of a given volume of gas, they must double the whole absolute quantity of heat in the gas. The specific heat of the gas at 512° will be double that of the pint at 32° in the experiment. Now this is a point which probably no one acquainted with all that is known regarding caloric will maintain. We can hardly suppose that the whole specific heat of a gas at 32° (viz. that due to its capacity and temperature, and all the latent heat due to its gaseous state) is equal only to that introduced by the 480 degrees. Analogy would teach us, that it is, at least, three or four times as much. If then the 480 degrees of heat can effect as much expansion as the whole previously contained in the gas could, we are led necessarily to the conclusion that the latter is opposed, even in gaseous matter, by an attraction, so far as to have an effective repulsive force equal only to that subsequently introduced by 480 degrees of temperature, nay to much less, for the fact of the presence of this attraction being once established, between the gaseous particles, this force must be considered as operating against the heat subsequently introduced; and must lessen its effective power.

This argument I may illustrate in a more familiar manner. Let a cylindrical vessel half filled with any gas, nitrogen, stand inverted in a vessel of water, so that the liquid being on a level within and without the pressure on the gas shall be just that of the atmosphere. If the surface be two square inches, this will be equal to thirty pounds. Let an equal quantity of oxygen gas be added, and suppose it at first to remain under the nitrogen, and the vessel to be raised so as to preserve the same level in the water. The oxygen will now bear the whole pressure, and communicate the same to the nitrogen above it. Each will be pressed on with a force of 30 pounds. In the course of time, however, the two gases will become completely mixed. Each will occupy the whole vessel, the bulk of each being doubled; but the two together

not filling more space than before. Now, it is clear, each presses on the water and each bears one-half of the pressure of 30 pounds, so that the elastic force introduced with the oxygen gas has enabled the nitrogen to double its bulk under the pressure of the air; and has done no more. If instead of adding the oxygen gas, heat had been added to the nitrogen until its bulk had been doubled by expansion, it is manifest the 480° which effected this would have introduced as great an *effective* dilating power as that of the whole specific heat of the oxygen gas in the other case. But it will not be contended, that the whole specific heat of the oxygen gas amounted to no more than 480 degrees: for analogy would lead us to conclude, that the latent heat due to its gaseous state (including that of the previous state of liquidity) must greatly exceed this quantity, and if we add all the caloric of temperature, in a substance of a large capacity for heat, from the natural zero up to the temperature of the experiment, we shall probably underrate the quantity at three or four times 480°. The question then is, whence does it happen that 480° of uncombined heat could aid the expansion of the nitrogen gas, as much as four times this quantity entering with the oxygen? A reason, it appears to me, can only be found in the following explanation. The latter heat is so far opposed by a mutual attraction between the atoms of the oxygen, that its free effective elastic power equals only that of the 480° in the other case.

If then any inference can be drawn from the equable expansion of a gas from equal increments of heat, it is certainly this; that a powerful attraction subsists between the gaseous atoms, reducing the elasticity of their large quantity of specific heat, in so great a degree, as to leave an effective elasticity equal only to what would be due to one-third or one-fourth as much heat. But the attraction cannot be apparent, because it is veiled beneath the excess of the elastic power.

The alleged fact that all gases have the same ratio of expansion has also been proposed as an argument against the existence of attraction between particles in a gaseous state. It is said that all gases have the same ratio of expansion, because the force opposing expansion is the same in all, namely, the pressure of the air; and that if an attraction be admitted between the particles of a gas it must be considered as equal in every gas, for otherwise the ratio of expansion would not be the same in all, and hence that there exists no attraction, for it cannot be considered as equal in all gases. Mature reflection will perhaps induce a different view of the subject. Though a certain change of temperature may produce an equal change in the mass of all gases, yet the separation of the particles may be scarcely the same in any two, for we

have no proof of different gases having the same number of particles in equal bulks.

In the combination of gases, a comparison between their prime equivalents, their proportions by volume, and the resulting bulks of the compounds, would lead to the conclusion that the number of particles in a given bulk differed materially in different gases. Thus, if it be assumed that in oxygen and nitrogen the number of atoms in a given bulk of each is equal, since one volume of the former combines with one of the latter to form the nitric oxyd gas, it would follow, that an atom of each unite to form each particle of the compound gas. If, then, in the latter, it be assumed, that in a given bulk the same number of compound particles exist as of simple ones in either of the former, it is clear that the two volumes ought in combining to condense into one volume, since two atoms form one compound particle. But experience shews that no condensation takes place. Therefore, whatever number of simple atoms have combined to form a compound particle, in the same proportion must the number of the latter in a given space have decreased.

Many other combinations of gases would prove equally hostile to the supposition, that all gases are at the same temperature and pressure equally dense. Hence, though equal rises of temperature may increase the bulks of different gases equally, the separation of the particles may differ in all. And further, the capacities of gases for heat differ materially. If equal bulks of hydrogen and olefint gases be taken, since their relative capacities for heat are as 1 to 1.7 nearly, we shall have these numbers representing the relative quantity of heat by each degree of temperature. It would require 1.7 of heat to expand an equal bulk of hydrogen. Since the pressure on each is equal and increases equally, whence does this arise? We are compelled, I think, to conclude that atmospheric pressure is not the sole force opposing expansion, but that it is aided also by an attraction subsisting between particles in the gaseous state, more powerful in olefint gas than in hydrogen, whence to effect an equal expansion more heat is required in the former than in the latter.

If then any inference can be drawn from the equable expansion by heat of different gases, it is this, that in every gas an attraction subsists between the atoms; but in some gases, as might have been expected, more powerful than in others.

By the above elaborate inquiry, I trust I have shewn that the facts usually brought forward as evidence of the limited distance to which

contiguous attraction is supposed to extend do in reality lead to an opposite conclusion.

Of an attraction between gaseous atoms, both similar and dissimilar, we shall have further satisfactory proof by the consideratiou of the following phenomena. In the transition of aqueous vapour to the solid state, a number of particles, which must have occupied a considerable space, convene to form a flake of snow.

This must surely have been produced by a general attraction throughout all the particles of that portion of vapour, the attraction between the contiguous particles being doubtless the most powerful. Hence each minute crystal of the flake is formed by the affinity of several neighbouring particles, but the aggregation of all the crystals to form the mass must be the product of an universal attraction of all the particles of the vapour. Otherwise no flake would be formed, but each grain would be precipitated separately. This instance alone appears a conclusive proof. Between dissimilar particles there are many like instances. The deliquescence of a salt has been adduced by Newton himself in proof of its attraction "acting at a distance" on the particles of vapour in the air.

The mutual action of the particles of different gases on each other is often evident at considerable distances, as when two gases combine to form a solid or liquid, such as the muriatic acid and ammoniacal gases, and many others.

If all these arguments and facts be admitted as true, sufficient has been said to prove that the attraction of atoms, whether of similar or dissimilar atoms, is not merely a contiguous force; and as we have had evidence of its being exerted by all atoms in a gaseous state, but have no proof of its ceasing at any point, it must surely be considered as a power that operates, though weakly, at a distance, and that it does not suddenly cease any where.

2ndly. *How far the attraction of atoms is general,* i. e. whether all atoms in nature attract and are attracted by all, or whether attraction between atoms, chemical and cohesive, is confined to a limited number.

It would seem to be the opinion of most modern philosophers, that all homogeneous atoms exert a mutual attraction when sufficiently near to each other, and hence that the particles of gases would cohere if brought within the limits of their attraction. That all homogeneous atoms attract each other, there is not any reason for doubting. It has been above shewn that we have no proof of a limit to the distance at which attraction may be exerted, and that even in the gaseous state

all particles must be supposed to attract each other. Since then in liquids and solids also an attraction is always manifest, it follows that between homogeneous atoms this force is universal. We have equal reason to admit its action between all heterogeneous atoms, though it has until lately been considered to exist only between a limited variety.

The fact that many atoms refuse to combine may be readily explained, as Dr. MURRAY has observed, by taking all the forces that oppose combination into consideration. These forces may in many instances be superior to that of the attraction, and then the latter will apparently not exist. A very strong proof of the universal action of attraction between dissimilar atoms, and even when in the gaseous state, is afforded by the fact that all gases without any exception will either combine, or else mix, when brought together; and further that all dissolve water when placed over it*.

The reason of an attraction being universally apparent between all gases, though not between all liquids and solids, is *readily* explained. In the former state, the particles of the body are not detained by any cohesion, but exert an effective repulsion† for each other, which renders them easy to be put into motion; hence even a weak attraction exerted by another gas becomes evident. On the other hand, the particles of solids and of liquids, on a small separation from each other, are detained by their cohesion, it being stronger than the attraction of many bodies for them.

With respect to the attraction, which acts between atoms, I trust that under the present head sufficient has been shewn, to justify its being considered as a power, which is universal, i. e. which is exerted (though with various degrees of force) between all particles similar and dissimilar.

3*dly*. *The effect of mass on contiguous attraction.*

If the statements, laid down in the two former heads, be true, it follows of necessity, that attraction must also vary with the mass, or number of attracting atoms; and this is confirmed by experiment, with respect to heterogeneous atoms. Thus it is well known, that a particle of sulphuric acid has a stronger attraction for one of potash than one

* It is well known Mr. DALTON and others have endeavoured to explain these facts, without the assistance of an attraction. This will be *discussed in a future part* of this paper.

† I have used the term effective, here and elsewhere, to denote the excess of one force above its opponent; thus, if the attraction be 4 but the repulsion 10, the effective repulsion = 6. In like manner, there is in some cases an "effective" attraction.

of nitric acid has. Let the force of the former be 8, that of the latter 4. If a compound atom of sulphate of potash were in this case exposed to three atoms of nitric acid, the potash would be separated, by the united action of the tbree atoms of nitric acid. In the same manner, the sulphuric acid may be taken from sulphate of barytes, by an excess of potash, as Berthollet has shewn. In both the above instances, mass evidently operates*. There is also every reason for believing, that this attraction varies as the mass, between homogeneous atoms, although there are not experiments proving that tbis is absolutely the case; for such experiments can hardly be expected, nor is it easy to propose a way of making them. In a homogeneous solid mass, this law does not plainly present itself, merely from the smallness of the atoms; from which, as formerly observed, tbe attractions of all those that are at a distance from each other (which is the case with far the greater part) becomes so much less than that of contiguous particles, (on which the solidity chiefly depends,) as not to admit of measurement with it.

The law of attraction which is here enforced, is also perfectly conformable with the doctrine of definite proportion, and does not in fact at all affect it, as has been by some supposed.

From all that has been stated, it must surely be admitted as a law of this power, that the attraction of atoms varies as their number.

4thly. *The ratio in which the force of attraction varies, and the identity of this power, with gravitation.*

I have observed, at the beginning, that the opinions of philosophers, upon the attraction of atoms, are various, and in many instances contradictory to each other. They are particularly so in the present question.

Among other theories is that of Boscovich, which is very generally known. In this it is supposed that atoms do not exert a simple power of attraction towards each other; but that their mutual attraction alternates with a mutual repulsion, not with variations of time, (as has been by some supposed of the affinity of bodies for ligbt) but with variations of distance. Thus that two atoms, when contiguous, repel each other with great force: and that this repulsion decreases with the increase of the distance, and at last vanisbes, giving place to an attraction, which increases with tbe distance to its maximum; whence it decreases, vanisbes, and is replaced by the repulsion, which obeys

* This fact does in no degree militate against the well established and important doctrine of definite proportions in combination.

the same laws. And that there are numerous alternations of these forces. According to this law, the particles of a mass must always remain at some one of the intervals between attraction and repulsion. This may be at various distances, and thus may be explained the various degrees of density, which the same body may possess at different times. To this hypothesis it may be objected that it cannot be easily admitted, of a *simple* force, that it should increase, as the centres of attraction are separated; much less then, that this force should suddenly, from a certain point, obey an opposite law, and decrease with an increase of distance.

But to admit, in addition to this, that the same atoms, from another certain point, exert an opposite force of repulsion, which obeys the same complicated law, and that these alternations are frequently repeated, until at last a regular decreasing attraction prevails, is scarcely possible; since it does not accord with the extreme simplicity always observable in the laws of nature.

Moreover, it is not possible by this theory alone, to account for the gradual increase of volume which bodies undergo, without introducing the repulsive agency of heat.

Though there are, according to this theory, many points of distance at which particles may rest, it cannot *of itself* account, even for expansion, much less for liquifaction and vaporization. And again, if the agency of heat be added to it, on a reduction of temperature, bodies would not contract in volume, for their particles would necessarily be prevented from approaching, by that region of repulsion, at the limit of which they lay. This would involve the necessity of another extraneous agent, namely some compressing force. And thus the two alternate forces, assigned in the hypothesis, are ineffectual without the assistance of the other two, and with them are altogether useless; consequently it is not philosophical to suppose them.

An anonymous writer in the Encyclopædia Britannica* treats of cohesion as a force, which extending to a small distance, is within this distance, " little or not at all altered by slight compression, or expansion." And in another place he says, " it appears, that the force of cohesion cannot be supposed to vary much with the density, and it is therefore allowable to consider it as constant as far as its action extends." I have, under another head, I think proved, that this attraction must not be considered, as extending only to very small distances; and the arguments, adduced in support of this, also prove, that attraction is a decreasing force. These are, the increasing ratio

* Supplement, Art. Cohesion.

of expansion in liquids, and the equable expansion of gases from decreasing additions of heat. The former can only be accounted for, by supposing that its chief opponent force, the attraction, *decreases*. The latter also requires the admission of an attraction between all gaseous particles, and that this force decreases likewise. For, did it not decrease, gases (as it was there demonstrated) could not expand as much from certain additions of caloric of temperature, as from their specific heat, so much more in quantity.

In Dr. Rees's Cyclopædia* we find another author, who expresses a very different opinion. "There is," he says, "an attraction, which is found to obtain in the minute particles, whereof all bodies are composed, which attract each other, at or near the point of contact, with a force much superior to that of gravity, but which, at any distance from it, decreases much faster, than the power of gravity."

And others, observing the apparently great decrease in the force of attraction, as particles are separated from each other, have supposed that it must vary as the inverse cube, or some higher power of the distance.

All these views have doubtless arisen, from attending to the apparent, rather than the actual, force of attraction. Since attraction, whenever presented to observation, is always opposed by a divellent force, the law of the simple force cannot be investigated by any direct experiment from its immediate effects.

There is however the strongest reason for concluding that contiguous attraction, as treated of in chemistry, is identical with the great universal power, gravitation.

This opinion has been hinted at by philosophers from an early age of this science, and among them by Sir Humphrey Davy†. But it may be demonstrated, as I think, in the most satisfactory manner, from the following considerations.

1*st*. The great Newton has demonstrated, that the gravitation, which prevails throughout the bodies of the system, is composed of the sums of the attractions between the atoms of the several bodies. And thus it is, strictly speaking, an attraction of atoms; and it is exerted between the same atoms as the attraction, which usually bears that name.

2*ndly*. It will be found to possess the same properties also.—First. That attraction of atoms, which constitutes gravitation, increases or decreases as the distance at which it operates is less or greater. This

* Art. Attraction. † Sir H. Davy's Elements of Chem. Philosophy, p. 68.

the same great author has shewn. For the attraction of a body in the mass (i. e. gravitation) depends wholly on this supposition. This same property we have seen* must belong to the other attraction of atoms. Secondly. The *absolute* force of gravitation varies as the mass. This, we have also seen†, must be a property of the attraction of atoms *chemically* considered.—Thirdly. By decreasing the mass, in gravitation, until the force operating only between a few or single atoms, this force would become imperceptible at a very small distance, which exactly agrees with the attraction of atoms in question‡. Fourthly. It has been above§, I think, clearly shewn that the attraction of atoms, as connected with chemistry, is universal; and is therefore in this respect perfectly similar to the attraction of atoms named gravitation.

We have here two forces exerted by the very same atoms, (namely those of which all masses in nature are composed,) and possessing the same properties, as far as a comparison can be carried on between them; and this extending through numerous particulars; whence we may conclude, that both are the same force differing only in the accident of distance, from whence it has acquired distinct names—and therefore, since, by the above-mentioned discovery of Newton, the forces of atoms composing gravitation vary inversely as the square of their distances, this force must still obey the same law, when considered under the name of contiguous attraction.

The truth of this doctrine, which I have been endeavouring to demonstrate generally, will I think be placed beyond all question, by the consideration of the following case.

A celebrated author, whom I have already quoted, has adduced the spherical figure of a drop of water in proof of cohesion operating throughout all its particles. Let us now suppose such a drop, *situated in absolute space,* to be enlarged by an accession of matter, until it became an ocean. This ocean would unquestionably retain the figure of a sphere; its parts being kept together by the same force, not at all changed in quality, but only increased in quantity. From having been once a drop, it would become a planet, and its attraction, which was called cohesion, would now be considered as gravitation.

In addition to this, it may be remarked, that part of the fluid, passing into vapor, would form an atmosphere around the planet, (admitting that it was exposed to the usual source of heat.) The force, which detained this atmosphere on the surface of the planet, would constitute its gravitation, which would be no other than the cohesive

* Vide page 443. † Vide page 451, Head 3. ‡ Vide page 443. § Vide page 450.

attraction. And, since it operates between the liquid and gaseous atoms, most of which are at a much greater distance from each other, than any two neighbouring atoms of the vapor, it at once proves that atoms in a gaseous state attract each other at all distances.

Part II.—*Of Repulsion.* Div. 1st.

Were the attractive force, which we have hitherto treated of, opposed by no other power, it is manifest, that the atoms of all bodies would be in perfect contact, and that all masses would be absolutely dense. Hence there must of necessity exist some divellent, or repulsive power in bodies; for the atoms of none can be in contact, since all are capable of contracting from certain causes. As they, in contracting, occupy a less space than before, the difference between their present and former bulks must have intervened between their atoms; and even much more; for no limit has been found to the contraction of bodies. It is owing to the same divellent power, that heterogeneous atoms cannot come into contact. Hence the limited number of combinations; and hence it happens, that most gases, on being presented to each other, merely mix, and cannot enter into combination.

Since a divellent or repulsive force is always as evidently operating to prevent the contact of atoms, as an attraction, exerted by them, is operating to favor their contact, the former has no less commanded the attention of philosophers, than the latter.

Any theory, which would at all admit of investigation, must suppose the great opponent force to the attraction of atoms to depend, either on a repulsive power inherent in and exerted by them; or on this force, aided by the power heat;—or on the power heat alone.

These I shall attempt to investigate severally.—*First.* Whether the opponent force to the attraction of atoms is a power inherent in and exerted by them.

The theory of Boscovich and a few others may be placed under this head. His theory, as above observed, would sufficiently account for the constitution of bodies, if their volumes were permanent, and their particles always at rest. But, since all bodies are capable of possessing every degree of density, and of expanding and contracting gradually, such a theory would interfere with known phenomena, which could not take place on the admission of it.

Bodies, as I have before remarked, would never expand without the introduction of some extraneous expanding power, nor could they contract, without the admission of a compressing force, of which we have no evidence, and the action of which could not be explained.

In short, as all powers inherent in atoms must be permanent, and as a permanent repulsion cannot alone account for densities and states, which are not constant, the power opposing attraction cannot be solely a power inherent in atoms.

Secondly. Whether the opponent force to the attraction of atoms depends on a power exerted by them, aided by the power, heat.

In a modern treatise on attraction* and repulsion, it is thus asserted :—" The states of elastic fluidity, solidity, and liquidity, in all of which the greater number of simple bodies are capable of being exhibited, at different temperatures, are not uncommonly conceived to depend on the different actions of heat only, giving a repulsive force to the particles of gases, and simply detaching those of liquids from that cohesion with the neighbouring atoms which is supposed to constitute solidity." And he adds, " but these ideas, however universal, may be easily shewn to be totally erroneous : and it will readily be found, that the immediate effect of heat alone is by no means adequate to the explanation of either of the changes of form in question." " There can never be rest, without an equilibrium of force, and if two particles of matter attract each other, and yet remain without motion, it must be because there exists also a repulsive force, equal, at the given distance, to the attractive force."

To this I answer.—It is undoubtedly true, that, to enable the particles of a body to be at rest, the opponent forces, operating on them, must be in equilibrio. And the remark, just quoted, might properly be objected to those writers who have treated of the force of attraction between the particles of solids, as being greater than the repulsion. But, since the question is, whether or not *heat* be the repulsive power which keeps bodies in the gaseous, the liquid, and the solid state, this remark cannot be considered as a proof on either side, since it has no reference to this question.

Admitting heat as the sole source of repulsion between atoms, its force may easily, nay must be considered, as equal to that of the attraction, whenever particles are at rest. The opponent powers must be in equilibrio, whether heat be the source of repulsion, or not.

In the same treatise also, *attraction and repulsion*, it would appear, are considered as being both exerted between atoms, at all distances within a certain limit. In the first place, it cannot be admitted as possible, that at the same distance, the same particles should at once attract and repel each other. But even supposing it possible ;—if this repulsion

* Encyclopædia Britannica, Supplement, Art. Cohesion.

be equal to the attraction in the liquid state, since it must be a permanent force, the attraction, being always opposed by an equal force, would never be able, under any circumstances, to draw the atoms into the solid state.

If the repulsion be considered equal to the attraction, when particles are at rest in the solid state, no solid could contract, unless exposed to an extra-compressing force, of which (as before remarked) we have no evidence, and which must only operate at certain times, for otherwise no solid could ever expand.

Since then it has been shewn, that, if an inherent repulsion, exerted by atoms, be considered, as one of the great opponent forces to their attraction, it necessarily involves the introduction of an extra-compressing force, which must only operate at certain times; and since no such compressing force can be demonstrated, it is manifest that such a repulsion cannot be considered as one of the opponent forces to atomic attraction.

Thirdly.—That the opponent force, to the attraction of atoms, depends on the power heat alone.

It has been already proved, that no inherent force of repulsion can be supposed to be exerted by atoms, and that such a force would not account for the phenomena of repulsion, which could not take place on the admission of it. It therefore follows, according to the division, that in heat consists the great opponent force to the attraction of atoms.

It is manifest, that previously to an attempt to explain the action of heat, as the source of repulsion, a decided opinion should, if possible, be formed of its nature.

The difficulty of this is apparent, in the fact, that chemical philosophers are divided between the two opinions, that the phenomena called heat depend on vibratory motions in the particles of bodies, or that heat is a subtle highly elastic fluid pervading all bodies.

1. That the phenomena of heat depend on vibratory motions, in the particles of bodies.

The phenomena of heat are of two kinds:—Those, which are apparent to the senses, and commonly called heat; and those of repulsion. The great philosopher Bacon, being unacquainted with most of the facts proving the repulsive force of heat, could only judge of its nature by those of the former kind. He, observing that great heat was produced by the friction and percussion of many bodies, that iron may even be rendered red hot by percussion, was led to the conclusion, that heat consists in a motion in the particles of bodies. But he did not apply his hypothesis to the explanation of repulsion. Of late years a great

philosopher* has extended the views of BACON, and has endeavoured to explain all the phenomena of repulsion by a vibratory and rotatory motion in the particles of bodies. This great and meritorious author writes in these words :—" When any body is cooled, it occupies a smaller volume than before, it is evident, therefore, that its parts must have approached towards each other ; when the body is expanded by heat, it is equally evident, that its parts must have separated from each other. The immediate cause of the phenomena of heat then is motion, and the laws of its communication are precisely the same, as the laws of the communication of motion." Since all matter may be made to fill a smaller volume by cooling, it is evident that the particles of matter must have space between them, and since every body can communicate the power of expansion to a body of a lower temperature; that is, can give an expansive motion to its particles, it is a probable inference, that its own particles are possessed of motion : but as there is no change in the position of its parts, as long as its temperature is uniform, the motion, if it exist, must be a vibratory or undulatory motion, or a motion of the particles round their axes, or a motion of particles round each other." And, he continues, " It seems possible, to account for all the phenomena of heat, if it be supposed, that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space; that in fluids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances, the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity on the motion being performed in greater space ; and the diminution of temperature, during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes, at the moment when the body becomes fluid or aëriform, or from the loss of the rapidity of vibration, in consequence of the motion of the particles through greater space."

It is under the deepest impression of respect for the author that I allow myself to make my observations on the doctrine supported in the above quotation, which observations are only stated from a persuasion of the importance of one decided and general opinion as to the nature of heat in forming the science of chemical philosophy.

* Sir H. DAVY's Elements of Chemical Philosophy.

It is certainly true, that when a body contracts on a reduction, or expands on a rise of temperature, in the one case the particles approach, and in the other recede, from each other. This approximation, and separation, is a gradual and regular motion. Thus, if two particles are kept at a certain distance from each other, by any force (whether of heat or not) on the removal of that force the particles must have motion, if they approach; but when they arrive the distance at which they are to remain, this motion ceases, and is no proof of vibratory motions in the atoms, nor can it give rise to them. When particles approach, they are put into gradual motion, by the force of attraction; and they will not separate, until a superior force urges them in a different direction.

Since the particles of matter have space between them, and since they exert great attractions for each other, the force, which keeps them asunder, must be equal to their attraction. If this force is a vibration of the particles, it cannot be permanent. No motion can be lasting, when opposed by any force, however small, unless it is preserved by an equal force. But the vibration of atoms would be opposed by a very powerful force, their mutual attraction; which would urge them into absolute contact; when any vibration must cease, from their impact against each other. It cannot be said, that their motion is kept up by that of neighbouring bodies, for the vibration of all particles in nature would very soon cease for the same reason.

The expansion, which a hot body produces, in one of a lower temperature, arises from the divellent power becoming superior to their attraction, and producing a slow and progressive separation of the particles of the latter, which power, as above shewn, cannot depend upon a vibratory motion, for any such motion must soon cease. And even could such motion last, it would not be increased by superior vibrations in another body, but lessened. If two vibrating bodies are brought into contact, their vibrations cease directly, from the one body being a mechanical obstacle to any motions in the other. But, if the motion in the one is greater, it will still more check any motion in the other, not only from the obstacle arising from its contact, but also from its increased impact, unless it be supposed, that the particles of the two bodies happen to be moving in the same direction, at the instant of their contact. This, which would involve the idea, that all particles in nature are always oscillating in the same direction, at the same moment of time, is moreover contrary to a supposition in the above theory, that bodies of different temperatures vibrate with different velocities, from which their atoms would soon move in different directions at the same time.

That the particles of solids are in a constant state of vibratory motion is incompatible with their mutual attractions, and their gravity. If temperature depended on vibration of atoms, bodies would soon have no temperature, (i. e. fall to natural zero,) for their particles would soon cease to vibrate.

This theory cannot explain temperature ; for bodies would lose their temperature if temperature be vibration. Nor capacity, if capacity be latitude of motion. Nor could radiation take place, if radiated heat be vibrations communicated through the air, for according to this theory, the particles of elastic fluids move with the greatest quickness. Thus, suppose the particles of any body A, are vibrating at any given rate 10, and those of another distant body B, at any less rate 8, as the air between them is vibrating with the greatest quickness, let its rate be 20. If the air vibrating at the rate 20 does not increase the rate of vibration in A and B, how can it transmit from A to B the small difference of their vibration ?—or how can it receive vibrations from A, which vibrates at a less rate than itself. And moreover, as matter of some kind must be present to transmit vibrations, radiation could not take place through a vacuum, as it is known to do, unless the " subtle medium" of NEWTON* be supposed to exist, which is not a part of *this* hypothesis, and which, as will hereafter be shewn, is very nearly allied to the "matter of heat" of LAVOISIER.

That the repulsive force opposing attraction cannot be explained by vibratory motions, supposed to exist in the atoms themselves of bodies, has been, I trust, proved by numerous unanswerable objections.

2. That heat is a subtle, elastic fluid, pervading all bodies.

The doctrine of the materiality of heat has been adopted by the greater part of modern philosophers ; and the cause of its entering bodies, and separating their particles, has been explained in three ways:

First.—BOERHAAVE, with some other philosophers, attempted to explain the distribution of heat, solely by supposing that its particles are mutually repellent. Hence its perfect elasticity, which it was supposed would expand it equally through space, so that, in equal volumes of space, there would be equal quantities of heat, whether occupied by other matter or not. And hence he concluded that equal volumes of matter always would contain equal quantities of heat.

That this is not the case, is proved by experiment, for equal volumes of matter, it is well known, contain very different quantities of heat. Moreover, the argument itself is not sound ; for very dense bodies, between the atoms of which a powerful attraction subsists, would never

* Treatise on Optics, Query 18.

admit heat, until it was so accumulated in rare bodies, that their elasticity was superior in force to the cohesion of dense bodies, which is so far from being the case, that the elasticity of the atmosphere is evanescent in comparison with the cohesion of most solids.

Secondly.—In his Elements of Chemistry Lavoisier proposed another explanation of the action of heat, in these words :—" It is perhaps more natural to suppose, that the particles of caloric have a stronger mutual attraction, than those of any other substance; and that these latter particles are torn asunder, in consequence of this superior attraction of the particles of caloric, which forces them between the particles of other bodies, that they may be able to reunite with each other*."

This hypothesis, which treats of heat as a non-elastic substance, is liable to so many objections, that it has had very few advocates, and was probably relinquished by its great author. It is only necessary to remark one objection, which must have alone induced him to reject it. If the particles of heat had an attraction for each other so far superior to that apparent in the densest bodies, it is manifest, that it would not be diffused through all bodies, but would collect itself into masses absolutely dense, between the parts of which the atoms of no other bodies could possibly exist.

Thirdly.—That doctrine of the nature and action of heat, which has been much received of late years, and which was introduced by Dr. Cleghorn, is so satisfactory, and conformed so nearly to the phenomena of the actions and motions of heat, that it may be considered as the true explanation. This doctrine, as is well known, considers heat as a body, whose particles are mutually repellent, but attract those of all other bodies, with various degrees of force. Hence its perfect elasticity, and hence its presence in all bodies, but in various quantities in each.

Previously to making any further inquiry into the laws and action of heat, I propose to weigh the facts, which have been considered as objections to its materiality, and to state various arguments in proof of its materiality.

The following facts have been at various times opposed to the material doctrine of heat:

1. That, when many bodies are subjected to percussion, much heat is evolved. Iron may even be raised to a red heat. The explanation of this, which has been given by others, does not perhaps place the fact in quite so clear a light, as the following :—Since the force of cohesion in iron is very powerful, it is plain, that the heat between its

* Elements of Chemistry, translated by Kerr, page 72.

atoms must be compressed with great force, and must exert an equal repulsion. If the compressing force is suddenly increased, so also must the repulsion, the iron being somewhat condensed. But, when these forces become superior to the affinity, which detains the heat in the iron, it is manifest that part of the heat must leave the iron, and this will take place until the affinity for the remaining heat becoming very great, little or none can be evolved, and the density cannot be increased.—This explanation is verified by the experiment. Less and less heat is evolved, at every succeeding blow, until at last little or none can be driven out, and here condensation ceases.

2. That much heat is made sensible by the friction and attrition of many bodies.

Since the particles of heat attract so powerfully, the atoms of all other bodies, as to enter even the densest, much more then will they be accumulated on the surface of bodies, and endow them with a repulsive force. Hence the fact that two plates of glass cannot be brought into contact, as Newton has shewn*. But if two bodies, rubbing againste ach other, have this superficial heat compressed, with a force superior to that which detains the *most distant* particles of it (which from their distance must be weakly attracted), it must happen, that part of the heat will be separated, while the friction lasts, and will be renewed as soon as it ceases. This explanation, which I have given of the fact, appears to render it perfectly conformable with the material doctrine of heat. As, in attrition, both the forces of friction and percussion on compression operate, there will be a double cause for heat becoming sensible, which has been just explained under the two former heads. The experiment of Rumford, in which much heat was evolved, in the boring of metal, and yet the parts torn off appeared to possess their former capacity, has been sufficiently explained by Mr. Dalton in these words:

"The fact is, the whole mass of metal is more or less condensed, by the violence used in boring, and a rise of temperature of 70° or 100° is too small to produce a diminution in its capacity for heat. Does Count Rumford suppose, that if in this case the quantity of metal operated on had been 1lb. and the dust produced the same as above, that the whole quantity of heat evolved would have been the same†?"

3. The fact, that heat is evolved, in the sudden change of gunpowder, by explosion from the solid to the aërial state, has been considered as an objection to this doctrine of heat; for this appears contrary to

* Treatise on Optics, Query 31.

† New System of Chemical Philosophy, page 98.

the known law, that in a change from a dense to a rarer state, heat is not evolved, but on the contrary becomes latent. Though this is almost an invariable law, in a simple change of any solid A, into a gas A; yet if in becoming gaseous, A undergoes a change into another gas, B, an absorption of heat is not a necessary consequence; for the heat in the solid A may be sufficient to keep B in the state of gas, or may even be more than requisite, in which case some heat will be evolved.

Thus the oxygen, in the nitre of the gunpowder, during the explosion combines with the carbon and sulphur. The carbonic and sulphureous acid gases may not require so much heat for their existence in the gaseous state, as is afforded by the solid oxygen; hence heat will be evolved. If the experiments of Lavoisier and Crawfurd may be admitted as at all correct, they will prove the justness of this explanation.

Lavoisier inferred from his experiments, on the combinations of oxygen gas, that in nitre it retains $\frac{7}{8}$ of the heat, on which its gaseous state had depended. Crawfurd has stated the capacity of oxygen gas, as much greater than that of any of its compounds, and hence $\frac{7}{8}$ of its heat will be more than sufficient to supply the latent heat of the carbonic and sulphureous acid gases, formed in this instance.

The late experiments of MM. Clement and Desormes, if correct, would show that the capacity of carbonic acid gas is equal or superior to that of oxygen, and would increase the difficulty of the explanation by making the one offered inadmissible. It must however be considered, that no conclusion can be drawn with regard to the habitudes of caloric from instances of sudden and violent chemical and mechanical action. Thus no small part of the heat may be liberated by the resistance offered by the air to the sudden expansion of the gases formed. Whence much heat that would have been latent became caloric of temperature at the moment of the explosion, and whatever was extricated would be readily absorbed again from the air on the diffusion of the gaseous products of the powder.

Again, in so great a chemical change we cannot from any established law affirm, *a priori*, that heat should be either liberated or absorbed. Admitting the *capacity* for heat of the gaseous products to equal, or even exceed, that of the gases condensed in the nitre of the powder, it does not all follow that the *latent heat due to the gaseous state* of the former should equal that of the latter gases; and these appear in nitre to retain this heat, though solidified by the intensity of the affinities.

[To be continued.]

II.—*On Progressive Development in the cold-blooded Vertebrata. By* D. W. Nash, *Asst. Surgeon, Beng. Est. A. L. S. Corresp. Member S. A.*

Among the many important considerations embraced by the theoretical department of geological science, the question of the gradual transition of fossil remains in the strata which form the crust of our globe,—the supposed development of the forms of organic life in a progressive and ascending series,—and the application by analogy of this hypothesis founded on actual observations of that which has been, to the phenomena which are daily recognized in the present state of things,—are subjects of the greatest interest to the geologist and naturalist, while to the cause of science in general their elucidation is of considerable importance.

On these questions the most eminent authorities among modern geologists are divided, and though not perhaps so violent in the expression of their opinions as the Neptunists and Plutonists of a former day, the advocates and opponents of the theory of progressive development have entered with no little warmth into this interesting controversy.

Mr. Lyell in his Principles of Geology, in speaking of the conclusions arrived at by Sir H. Davy from the consideration of geological data, expressly states, that, "the theory of progressive development of organic life from the simplest to the most complicated forms, has no foundation in fact."

On the other hand many observers equally high in scientific reputation have imagined that they see, not only in the fossil monuments of former worlds, the imperishable evidences of a state of things differing from, and antecedent to, that now under our observation,—but also in the organization of the present inhabitants of our globe, indisputable proofs of a progressive advance to perfection in the forms of organic life.

It cannot be denied that the fossil remains which have been observed in the different strata of the earth's crust, are arranged very nearly in the order which the animals to whom they belonged, occupy in the natural system of zoology;—that those genera which zoologists are agreed in considering as the least developed forms are found in the lowest or most ancient formations, and that, as we ascend from the primitive through the transition, secondary, and tertiary rocks, new and more perfect forms of life meet us at every step of the investigation.

Setting aside the consideration of the order in which the development of the invertebrate classes has proceeded, as embracing too wide a field and requiring a minute investigation of the anatomical relations of this vast class of animals, the cold-blooded vertebrata appear to offer the most convenient opportunity of observing the analogies which subsist between animals of the same type of conformation, but differing in the degree of perfection at which their various systems of organs have arrived.

It appears that at one period of the earth's history—that in which the deposition of the secondary formations was taking place,—circumstances were highly favorable to the development of the cold-blooded tribes of vertebrata. The oceans swarmed with enormous cephalopoda, with gigantic individuals of a saurian race which has long since vanished from the surface of the globe, but whose remains scattered in such profusion through the oolitic group furnish the zoologist with data which enable him to fill up many apparent vacuities in the scale of the creation.

Nor, as might have been expected, if we determine to admit the present as the only true standard by which to judge of the state of things in past epochs, was this form of organization chiefly peculiar to the inhabitants of the waters; the ancient continents contained animals of this type only; the megalosaurus and the iguanodon peopled the forests; the banks of the rivers and fresh-water lakes were frequented by crocodiles and huge salamanders, while the pterodactyli pursued their prey amid the palms, the *cycadeæ*, and the tree ferns, of the primeval Flora. But not until after the deposition of the great calcareous formation do we find any trace of the existence of a warm-blooded animal: not even the most strenuous advocates for the uniformity of the past and present operations of nature have been able to prove that animal life had progressed so far as the development of the class mammalia, or of birds, until after the epoch just alluded to.

The only exception to be made with regard to this statement is met with in three or four specimens consisting of fragments of the lower jaw of an animal which has been pronounced by the highest authority to have been a species of didelphis. This fossil, discovered in the Stonesfield slate, a member of the oolitic series, lying below the cornbrash and above the Bath oolite, contains nine similar acuminated molares, terminating in three elevated points; but as no living didelphis possesses this number of molar teeth on one side of the jaw, and as those of the didelphis present the characters of insectivorous teeth,

it may be permitted to entertain a doubt as to the animal to which this specimen should be referred.

The shape of the teeth appear to indicate a carnivorous character in the animal to which they belonged, and bear a considerable resemblance to the molares of the seal.

Supposing this to be the case, the position of this fossil would not be, as Mr. Lyell imagines, as fatal to the theory of successive development as if several hundreds had been discovered, since its appearance is subsequent to the period in which the great Saurian reptiles were the most abundant; and should it prove to belong to the genus phoca or to some cetaceous animal, it would be an example of the commencement of the type of mammalia in one of the least perfect tribes of the order, and therefore an additional argument in favor of the theory it is intended to subvert.

In endeavoring to show that there actually does exist what has been called a stimulus of perfection in the organic world, it will be necessary to take a system of organs in its most imperfect form, and to investigate the steps by which nature has succeeded in effecting a series of gradual improvements.

Of the various functions conducing to the preservation of the individual, none is of more importance than that by means of which the oxygenization of the blood is effected, and this fluid rendered fit for repairing the waste of the body, and supplying materials for the growth and increase of the different organs. The development of the respiratory and circulating systems will necessarily be in a certain and constant ratio to each other, and, wherever we see a perfect respiratory apparatus, we have an indication of a proportionally complicated set of organs for the circulation of the blood, and consequently an increase in the irritability and nervous energy of the animal.

The respiration of the embryo in warm-blooded animals is at first solely cutaneous, and the heart consists of two cavities, both systemic, as no respiratory organs are developed. The systemic ventricle is then divided by a septum, and the right ventricle thus formed is prolonged into a tube which opens into the aorta subsequently to the origin of the branches which supply the upper portion of the trunk. This prolongation of the right ventricle is called the *ductus arteriosus*, and from it are given off small branches, which go to supply the lungs. The circulation is now that of a reptile, the heart in effect consisting of two auricles and a ventricle; but on the emergence of the animal from its fœtal state, the lungs become the immediate organs of respiration; the blood is more perfectly oxygenized; the irratibility of the

animal increased; the ductus arteriosus is obliterated; its pulmonic branches alone give a passage to the blood, the whole of which, now undergoing the necessary changes in the lungs, is sent from the systemic side of the heart to perform its functions in the animal system.

There is now therefore a heart of four cavities, and a perfect system of respiration, in short, that of the highest type, birds and mammalia. The first appearance of that form of organization which runs through all the vertebrated classes is to be found in the most perfectly developed tribe of the invertebrata, the naked *cephalopoda.* The chambered and convoluted shell of the nautilus and the ammonite may be traced in the internal skeleton of the sepia, which consists of numerous concentric lamellæ of carbonate of lime, connected by an infinite number of siphonculi running right angles to them. Now suppose each lamella separated from that next to it, and the number of connecting siphonculi reduced to one between each lamina, and a polythalamous shell will be produced.

Still higher we find in the *loligo* a single cartilaginous plate, somewhat concave anteriorly, as though its edges were approximating to form a tube, enclosed within the mantle, and lying posterior to all the organs of respiration, circulation, digestion, &c.. This cartilaginous plate performs the office, though imperfectly, of a vertebral column, forming an organ of protection for the nervous system. The carbonate of lime, so universal in the external skeletons of all the Mollusca, has here entirely disappeared, as though preparatory to the introduction of a new element characteristic of the skeletons of the higher classes, the phosphate of lime. By a very easy transition from this simple skeleton of the *loligo* we pass to the lowest of the cartilaginous fishes, where in the *petromyzon,* the vertebral column presents a form almost as rudimentory.

The respiratory and circulatory apparatus in the *loligo* are very nearly the same as in fish, being entirely aquatic; the aeration of the blood takes place in the branchiæ, placed on each side, hanging freely in the cavity of the mantle, and fixed on their dorsal aspect to cartilaginous laminæ, which may be considered the rudiments of branchial arches.

The blood brought by the venæ cavæ to two muscular cavities called auricles, and thence sent to the branchiæ, is returned to a third muscular heart, to which the name of ventricle has been given. There is here no essential difference from the circulatory organs in fishes, but a lower degree of development is indicated in the permanent disunion of the muscular hearts, a concentration of organs being one of the most characteristic features in perfection of development.

In the most simple of the cartilaginous fishes the vertebral articulations are not distinguishable; the spinal column is little more than a cartilage through which are dispersed granules of phosphate of lime, and even in osseous fishes the proportion of earthy matters contained in the skeleton is comparatively small.

The normal form of the vertebræ in fish is, a cylindrical body with two concave, cup-like articulating surfaces; the interval between two vertebræ being filled up by a fibro-cartilage, which of course presents two globular surfaces corresponding to the cavities of the vertebræ: this circumstance is of considerable importance, as we shall be able to show the steps by which a transition from this form, typical in fish, to the vertebra of a reptile has been effected.

The lateral development and extensive mobility of the intermaxillary bones are also worthy of observation, as the same characters obtain in the next class, the Batrachia.

In the *petromyzon*, the nervous system exists in a very rudimentary condition—very much in the state in which we observe it in the embryo of the chick; two delicate cords, placed along the back, and giving off from their sides other nervous filaments.

The two nervous cords developed in the embryo upon the serous layer of the germinal membrane diverge anteriorly to enclose three spaces, which being afterwards filled up by cineritious matter become the medulla oblongata, the optic lobes, and the hemispheres of the brain. In the class of fishes the optic lobes, dedicated to the supply of organs of sensation merely, are nearly double the size of the hemispheres; but as we ascend in the scale, the latter become gradually larger and extended backwards in proportion as the former are retarded in development, and also in some indefinable ratio to the power and extent of the intellectual faculties.

The organs of respiration in the class of fishes are always branchial, but present some differences in the two great divisions of the order, the cartilaginous and osseous fishes. In the latter the branchiæ, formed by innumerable ramifications of the branchial arteries, hang suspended from the branchial arches, having their outer edges free and movable. The water which is drawn into the mouth by the action of the os hyoides and branchial arches, passes over these vascular follicles, and escapes by an opening common to all the branchiæ of one side, and defended by a valvular structure composed of an opercular membrane and a bony operculum.

In the cartilaginous fishes, on the contrary, with the exception of two families, the sturgeons and the chimeras, the branchiæ, instead of

having a free margin, are fixed, being connected with the integument by their external border. The consequence of this conformation is, that the water which passes over the branchiæ makes its exit through distinct canals opening on the surface, whose number varies from four to seven in different genera of the order.

In all this may be observed an evident tendency to a higher degree of development, an attempt on the part of nature to cause the respiratory apparatus of the most perfect of the class of fishes to assume the appearance of that possessed by the most inferior among reptiles, and the next step will be to inquire whether there is not to be found some intermediate state between the two.

The larva of the common frog is, during its larva condition, *bonâ fide*, a fish; its respiration is aquatic; its circulation double; it possesses four branchiæ on each side, suspended from branchial arches, not enclosed however by an operculum as in fish, but hanging free from each side of the neck. The heart consists of two cavities, an auricle and a ventricle; the whole of the blood passes through the branchiæ by four branchial arteries on each side; it is returned by as many branchial veins, which afterwards unite to form the abdominal aorta. This circulation is strictly branchial not systemic, and is in every respect the circulation of a fish. During this fish-like condition of the larva, the spinal cord presents no enlargements in its course, and extends down through a number of coccygeal vertebræ; at this period also the optic lobes are larger than the hemispheres of the cerebrum, as in fish.

This then may be considered to be the intermediate point of development between two series of forms of animal life, and here is the stage from whence to set out in marking the changes which are required to render, not only the same type, but the same individual capable of exercising its functions in a medium very different from that in which it originally existed.

After remaining in its icthyoid condition for an indefinite period of time, the duration of which is influenced by a variety of circumstances immediately affecting the development of the animal, as temperature, the action of light, the abundance or scarcity of food, &c. the tadpole begins to undergo certain changes, which are the prelude to a complete metamorphosis; changes which are to give it the organs and habitudes of a land animal, and enable it to act a part in a situation totally foreign to that to which it has been accustomed.

This first of this series of changes takes place in the nervous system. The direction of development, which has hitherto been longitudinal, becomes lateral; the spinal cord shrinks up, and the coccygeal verte-

bræ of the tail are gradually absorbed; enlargements of the cord are evident at the points where the organs of locomotion are to be produced, and shortly after these organs begin to display themselves.

At the same time an important change takes place in the relative magnitude of the hemispheres of the brain and the optic lobes. In fish, as before stated, the optic lobes are the larger, the hemispheres having attained but a very inferior degree of development; during the metamorphosis of the larva, the latter rapidly increase in size, till they have become considerably larger than the optic lobes; the olfactory tubercles are no longer separated from the hemispheres; the whole cerebral mass having assumed a more concentrated form, instead of presenting the appearance of a number of imperfectly united ganglia.

In speaking of the mode in which the blood circulates in the larva, I described four branchial arteries on each side, passing to as many respiratory organs, and conveying to them the blood which is to undergo the process of oxygenization.

Synchronously with the change which the nervous system undergoes, this mode of circulation experiences considerable and important alterations;—the anterior branchial arteries, which are so many subdivisions of the aorta, are obliterated—the posterior branchial artery alone remaining pervious; while its numerous ramifications are reduced to a single trunk, the union of which with the artery of the opposite side forms the trunk of the abdominal aorta. From the thoracic aorta is given off on each side a small pulmonary twig, which now becomes the channel through which the blood passes to the organs of aeration.

Another remarkable circumstance is the change which now takes place in the form of the vertebræ. The vertebra of a fish, we have said, presents two cup-like articular surfaces, the space intermediate between two vertebræ being filled up by elastic cartilage. The vertebræ of reptiles always present one convex and own concave articular surface, the globular head of one vertebræ fitting into the concavity of the one immediately below it, so as to form a ball and socket joint. While in the tadpole state, the vertebræ of the animal resemble those of fish; but it was observed by Dutrochet, that, at the period when the change in the respiratory apparatus was going on, the intervertebral substances became ossified, each uniting itself to the vertebra immediately preceding.

When the metamorphosis has been fully accomplished, the lungs of the adult animal are found to be tolerably perfect, but still not so minutely cellular, and consequently not presenting so extensive a surface for the aeration of the blood as in the higher reptiles. Never-

theless, the respiratory organ would appear to have made its appearance in so perfect a form rather suddenly on the stage, if we were not able to trace it progress towards perfection from fishes themselves through other members of the Batrachian tribe, up to the point where we have seen it completely formed, and capable of exercising all its functions in the adult frog. For this purpose we must return to our examination of the class of fishes.

All fish, with the exception of the genus *pleuronectes*, are furnished with an air-bladder, for the most part entirely isolated from any communication with the atmosphere, and inflated with an aeriform fluid, secreted sometimes by the internal walls of the airsac itself, sometimes by a distinct glandular organ attached to it. The air contained in this bag is found to vary with the habitude of the animal, the quantity of oxygen being increased in proportion to the depth of water which it inhabits.

This air-bag, which is totally imperforate in the least perfect osseous fishes, is found to communicate with the external atmosphere in the most perfect osseous, and in the cartilaginous, fishes; in the carp it opens by a long canal into the stomach, in the sun fish and in the sturgeon it communicates with the œsophagus.

In the *proteus anguinus* and the *siren lucertina*, animals belonging to that division of the Batrachia called *perennibranchia*, from thè circumstances of their retaining their branchiæ and their aquatic mode of life during the whole term of their existence—we find two air sacs, very similar in appearance to the air-bladders of fishes, each communicating by a narrow membranous tube with the pharynx. Upon these sacs a minute branch sent off previously to the origin of the branchial arteries, is seen to ramify, but the influence which can be exerted on the circulation by this means is too slight to be taken into consideration.

Advancing one step higher in the scale, we come to animals which at a certain period of their life lose the organs of aquatic respiration, and breathe atmospheric air only by means of lungs—in short, undergo the metamorphosis we have been considering in the larva of the frog.

This change is first observed in the tritons or salamanders, belonging to that family of Batrachia which from the circumstances indicated has derived the epithet *caducibranchiæ*. In these animals the lungs still retain the form of simple sacs, in the upper and back part of which a cellular structure and more complex ramification of the pulmonary vassels begins to appear—a structure which is at length

perfected in the family of the Crocodilida, where the most complete system of respiration obtains among reptiles.

It has now been shown, that in the two great systems which exercise the most important influence over the development of the animal, there is a gradual and well-marked progression towards perfection in the organs by aid of which these functions are performed ; and that it does not require the aid of the imagination to trace the steps by which the simple air-sac of the sturgeon has passed through the intervening stages in the proteus and the triton to the elementary lung of the frog and the more perfect organ of the crocodile.

It will be as easy to show that the same system of gradual progression has been followed throughout all the members of the series : the links which unite Batrachia with the Ophidian reptiles, and these latter with the Saurian tribes, are too evident to render necessary a lengthened detail.

In the genus *cæcilia* we are supplied with the form which connects the Batrachia with the serpent race. The auricle presents a partial septum, an indication of the change to be effected in the heart of the true serpents, where there are three distinct cavities. In the arrangement of the teeth upon the maxillary and palatine bones, the *cæcilia* resembles the proteus, but in the shape of the teeth comes nearer the true Ophidia.

With regard to the respiratory organs, the left lung is, as in serpents, retarded in development. The skin is soft and naked as in Batrachia, but according to Baron Cuvier, it contains, within its substance, small scales regularly disposed in transverse bands. The true serpents are separated from the Saurian reptiles by the total absence of any vestige of sternum or extremities ; this is the most prominent character, and will therefore be the most easily traced.

If we passed at once from Ophidia, where extremities are totally wanting, to the lizards where they are perfectly formed, we might suppose that there had been a sudden production in one order, of an organ, of which we had observed no elementary condition in the order immediately preceding ; a circumstance entirely at variance with all that has hitherto been observed.

But in this instance, as in every other, there have been successive stages through which the organs of locomotion have passed. There is a small family of reptiles placed between Sauria and Ophidia, in whom these organs are seen to be gradually developed. In the *anguis* and the *ophisaurus* a rudimentary sternum and pelvis are concealed beneath the integuments; in the *scheltopusik* a small femur has been added,

which here commences to display itself externally. From hence the gradual progress of the organ may be traced through the *chirotes,* the *bipes* and the *seps.*

In the same way may be seen the gradual increase in the size of the left lung which had been retarded in growth in Ophidia—and the progressive perfection of the organs of sense, of the osseous, and of the nervous systems. With regard to Chelonia, the highest in the class of cold-blooded vertebrata, the consideration of the numerous analogies which their anatomical structure shows to exist between them and warm-blooded animals, the commencement of a perfect division of the ventricle, and the evident transition from these animals to the class of birds, are subjects which would extend this paper beyond the limits of a brief memoir.

In the endeavour to trace the connection between these different tribes of animals, it is to be remembered that the materials for investigation are comparatively few ; that unacquainted as we are with the internal structure, and more minute anatomical relations of the extinct races, we are deprived of the evidence most material to our cause ; yet imperfect as our knowledge of these animals must necessarily be, we are able to trace in their analogies with existing genera, a type intermediate between two important divisions of the animal kingdom, and occupying permanently the station now held temporarily by Batrachia during their metamorphosis.

Examples of this kind, where the intermediate stages apparently wanting in our systems of zoology are to be discovered in the ancient strata of the earth, are very numerous. Among the fossil Echinodermata in the chalk formation, the gradation of development, from the flattened and ramified *euryale,* through the *clypeaster,* the *sentella,* the *ananchite,* the *galerite* and the *spatangus,* to the concentrated and spherical form of the *cidaris* and *echinus,* must strike the most cursory observer. The tertiary strata of the Paris basin, have furnished us with the links which were wanting in the order Pachydermata to fill up the hiatus which separated the pig and the tapir from the elephant.

If these observations be correct, no organ or system of organs, nor any new type in the animal world, can be said to have suddenly appeared on the stage of existence. There are certain laws to which nature herself is compelled to submit, and by which all her operations must be regulated ; and notwithstanding the weight which attaches to the opinions of the learned professor already quoted, I cannot help believing that amongst them is to be found the law of progressive development.

III.—*Some Geological Remarks made in the country between Mirzapúr and Ságar, and from Ságar Northwards to the Jamna. By the Rev.* R. Everest, *F. G. S. &c.*

Mirzapúr is situated on a kankar bank on the southern side of the Ganges, and somewhat higher above the level of the water than these banks usually are. The steep side of it, towards the river, shows a section of strata similar to what is usually observed in this formation, viz. beds of clay and calcareous marl of different colours with nodules of limestone imbedded in them. The lowermost of these beds exhibit some inclination and faults in particular places, which indicate that they have suffered some disturbance since their deposition. Upon these the upper beds rest horizontally and unconformably. One or two casts of shells (apparently fresh-water) and some small fragments of vegetable stems, were the only remains I could observe. But the appearance of the kankar nodules here marks more strongly their origin than in any place I have yet seen. They are mostly of the form of stalactites, from the size of a finger to that of a wrist in thickness, and, when broken, shew a compact, splintery, bluish-grey limestone, with occasionally minute scales of silvery mica disseminated through it. Occasionally too they are dependent from the roofs of small cavities in the clay-beds, and at other times spread out into layers, so as to form a complete seam of limestone. Before quitting the subject of kankar, I wish to notice a remark I have sometimes heard made, that probably the formation of kankar is yet going on. Mr. Piddington alludes to this in his remarks on the silt deposited by the river Huglì, and from his analysis it would appear that the quantity of carbonate of lime in the silt is considerable. That kankar may be yet forming in many places where calcareous springs are now running, cannot admit of doubt; but that it is at present depositing from the waters of the Huglì or Ganges I am inclined to disbelieve. For, were this actually the case, we might expect to find kankar on low tracts that had been flooded, after the retiring of the annual inundations; whereas the very reverse of this happens. As far as my experience goes, kankar is never found on the low grounds that are inundated. On the contrary the kankar banks are the only parts of the country that remain several feet above the level of the highest floods*.

* Considerable deposits, however, of saline matter are to be found on lands overflowed by the Jumna, when the rains are over; which, of course, are a recent formation: but the saline deposits, as I have noticed elsewhere, are usually above the present level of the floods.

At the distance of four or five miles to the south of *Mirzapúr* we come to the sandstone range, about 200 feet high, and presenting a steep escarpment to the alluvial plain at its base. Thence it sweeps round in a N. W. direction to *Vindáchal*, where it may be traced nearly to the bank of the river. The front of it towards the water is covered with rounded boulders nearly to its summit. From hence this range extends to *Chunar*, as may be seen in Captain Franklin's map, and east of that to a place called *Jemorah*, where I have before mentioned it as occurring. It preserves here the same character as at that place, viz. that of a small-grained, highly consolidated sandstone approaching to quartz rock, usually of a greenish grey or faint pink colour, and splitting into large slabs of divers thicknesses. At *Vindáchal* the general dip is to the west at an angle from 5° to 20°. Further to the east, where the road to *Ságar* ascends it at the pass of *Tárá*, the dip is to the west, and scarcely perceptible. At the *Tárá* waterfall a deep section may be seen of it. It presents no variety of character, nor is it at all interstratified with marls or shales. At the foot of the pass I found an efflorescence of soda on a kankar bank, similar to what occurs in the plain to the N. W. of *Ghází́pur*.

After ascending the pass we travel over a country nearly flat and covered with soil and vegetation. About 20 miles further on, at *Lálganj*, the rock was laid bare in the bed of a small nullah dipping slightly to the north. The soil above it contained pieces of kankar and iron ore, similar to what occurs about *Bankúra* and elsewhere. Nine miles further on in the bed of the Bálan river, the rock was exposed with a slight dip to the west. At the foot of the *Kattra* pass (for the situation of which I beg to refer to Capt. Franklin's map, (Trans. Phys. Cl. vol. i.) I met with soda efflorescing, and kankar, at the side of a ravine, as I had done before at *Tárá*. From *Kattra* the road winds up a precipitous ascent over strata of sandstone dipping to the N. W. The sandstone does not appear to differ from that of the lower platform from *Kattra* to *Tárá*, but it is here interstratified with thick beds of red and greenish-grey marl-slate, and rarely with thin layers of a rock resembling greywacke, rather dark-coloured but containing pieces of slate imbedded. At *Mowganj*, two marches beyond *Kattra*, the dip of the rock was N. E. at an angle of from 10° to 15°, as seen in the bed of the nálá. At *Lour*, a little further on, they were quarrying a slaty marl, with shining facets and white streaks running through it. These streaks are calcareous and effervesce strongly with acids. Pieces of a compact splintery limestone are also to be found lying about on the surface. The strata here are horizontal. In the

Pakaríga nálá, between *Lour* and *Mangowa,* we first came to a thick slaty limestone, generally whitish, earthy, and marly, and varying to yellowish, greyish, and fine splintery. At *Mangowa* the dip was to the north, a red slaty marl. About this part of the country we begin to see a distant range of hills, bounding our prospect to the south and south-west,—the *Kymur* hills; judging from the outline, they appear to be sandstone with a horizontal stratification, and look as if a third platform or table-land existed in that direction. Beyond *Raypúr* a low hill appeared to the south of the road, of a thick slaty limestone similar to that at *Pakkáríga:* the dip very slight and irregular; layers of a black kind of porphyry are interstratified with it. This black rock sometimes changes suddenly to white, and appears vitrified exactly like porcelain. At *Rewah* the limestone was extensively laid bare in the bed of the river, but it is here principally massive, passing from greyish to bluish black and black, and exactly resembling the mountain limestone of England. At *Rámpúr,* one march beyond *Rewah,* strata of red and variegated marl, most of them calcareous, were exposed in the bed of the rivulet for two or three miles to the south;—dip slight to the north. Beyond *Rámpúr* the same bluish black limestone appeared as at *Rewah.* At *Patráhat* a similar limestone was resting on the variegated marl slates, with a slight dip to the north. Near *Loháwel* we passed over horizontal beds of a crumbling green and red marl for a considerable distance. At *Nágowar* a similar limestone appeared to that at *Patráhat,* resting like it upon the marl slate. But it here appears to abound in what I believe to be coralline remains, I might rather say, to be entirely composed of them. I forbear describing them, as I have sent specimens with this paper, which can be examined by those who have means of reference at hand*. I was not fortunate enough to discover any of the stems of ferns and gryphite shells, described by Capt. Franklin; nor in my whole journey over this limestone did I meet with any other kind of organic remains than the one I have just now spoken of, though I made diligent search for them during a whole fortnight. They must, therefore, be extremely rare, and in this respect the limestone differs widely from any of the English limestones above the new red sandstone. From this place we passed alternately over strata of sandstone, red marl slate, and limestone, without being able to trace their connection with each other, until we came to *Hattah.* Here on the slope

* The specimens are deposited in the As. Soc. Museum: but their nature has not been ascertained. They are identical with what Franklin named "stems of ferns." See As. Res. xviii. p. 29.—Ed.

to the east of the village were horizontal strata of sandstone exposed to view; at the first nálá, lower down, was a whitish argillaceous limestone overlaid by sandstone; at a nálá still lower down, layers of sandstone, limestone, and red marl slate were to be seen interstratified. A few miles further on, at *Nagar,* a low cliff on the side of the river *Sonar* shewed a section of the strata as follows: uppermost layers, sandstone;—middle, red marl slate;—lowest (in bed of the river) argillaceous limestone. I had before conjectured that this would be the case from the continual alternations of sandstone and limestone, every mile or two along the road by which we had travelled, though both were horizontally stratified, and little or no difference of level was to be noticed. This led me to conclude that the limestone was of no great thickness, nothing more indeed than a bed in the sandstone, and the appearances I have now described at *Hattah* and *Nagar* confirm this. Capt. Franklin speaks of the limestone being not more than 100 feet thick upon the sandstone, I have never found it 10 feet thick, without layers of sandstone interstratified. Beds of limestone of a similar kind do not appear to be uncommon in this formation: near *Cheynpúr,* about 35 miles to the south of *Ghàzìpúr,* I had an opportunity of examining one of these. The sandstone range there presents nearly the same appearance as at the back of *Mirzapúr,* except that it is somewhat higher, and the dip, as far as I traced it, (which was about 20 miles to the eastward) is inwards, or to the south and southwest. At a place called *Mussaye* the limestone may be seen cropping out at the base of a sandstone hill, and dipping at a considerable angle to the south. It is usually slaty, but varies much in character in other respects, passing from grey to black, and then resembling the English mountain limestone. No remains could be found in it, but about 10 miles to the eastward it is seen again at *Bítrában*d. But I have neither seen, here nor elsewhere, any of the beds of loose slate and clay that accompany the lias in England. Were it necessary to class these with any of the European formations, transition limestone would be the most proper name for them. Though it is certainly more correct to consider them merely as beds in the sandstone; which sandstone, it must be remembered, is never found reposing on any but primitive rock.

As we leave *Patteria,* the easternmost extremity of the hills of trap fronts us, and the road winds along it for some distance. For 30 or 40 miles to the east of this the strata of sandstone had become broken and disturbed, dipping in various directions. Rolled pebbles of sandstone and pieces of agate and chalcedony are seen lying about, not

confined to the water-courses and lowest grounds, but extending over the highest ridges. Near *Patteria*, the bank of the *Sonar* shewed a section of a bed of pebbles several feet thick, containing fragments of shells of the genera *cyclas*, *paludina*, and *unio*. At *Uslúma* I observed a curious appearance, which would lead to the inference that kankar nodules and the soil in which they are imbedded were deposited on the sandstone at a time when the latter was in a state very different from what it is at present, viz. soft and flexible. At first sight it appeared that the kunkar and soil were interstratified with the upper layers of sandstone; but on looking further it seemed that both had come in from above through a fissure in the rock, and that the layers at the edge of this fissure had been bent downwards, as if by the superincumbent weight. Thus

B

A  A { A, layers of sandstone. B, kaukar and soil.

Now no pressure however applied is sufficient to bend a layer of sandstone in its present state. In the ruined palace of Akbar at *Fattehpúr Sikri*, many slabs of sandstone that have formed parts of the roof of the building may be seen broken asunder from long-continued pressure, but none of them, though there are many entire, are in the slightest degree bent.

At a short distance beyond *Patteria* the road passes over a white earthy limestone rock, containing sandstone gravel imbedded. This, in some places, loses all massive appearances, and becomes a collection of nodules not differing from kankar. They are however more white and earthy, approaching to the nature of chalk, than I have met with in the country to the eastward. As we advance, the peculiar outlines of basalt present themselves in the country round. The road soon crosses what has apparently been a stream or *coulée*, and has taken the lowest ground. It is dark-coloured, nearly black, and considerably cellular on the outside; yet this is an effect only produced by weathering; within, it is a solid hard basalt, of great specific gravity, and containing olivine imbedded. The surface of the soil in the country round is strewed with large round balls, resembling the volcanic bombs of volcanic districts: but they too, although scoriaceous on the outside, are, within, a solid basalt. With these are found abundance of agate and chalcedony. These appearances continue all the way to *Ságar*, and the rock does not differ in character, except that it sometimes becomes of a lighter colour, and is then in a high state of decomposition, crumbling under the hand. Three or four miles before reaching *Ságar*, where the road had been cut through the rock, a

ridge of basalt affecting the columnar form is seen resting upon a lighter coloured stratum, which shews by its state of decomposition, its great antiquity. From all I have been able to see or learn of this formation from others, it appears every where to preserve great uniformity of character, and resembles (as stated by Mr. LYELL when speaking of it on the banks of the *Nerbudda*) the currents of prismatic lava in Auvergne. Currents of porous lava, cones of cinders, scoria, pumice, ashes, all those products that peculiarly belong to modern volcanic formations, are wanting. We meet every where with a compact heavy basalt, with olivine sometimes and augite crystals imbedded, and agates, chalcedony, and jasper in great variety and abundance. And though some of the currents appear to have taken the lowest ground, yet their outlines are so worn down and effaced, and their surfaces are so deep in soil and vegetation, that it is difficult to assert even this with certainty.

About a mile distant from *Ságar* many white blocks appeared by the road side, which I at first mistook for a kind of trachyte, from the peculiar rugged appearance of them : add to this, that crystals are disseminated in the porous earthy base, looking just like the crystals of glossy felspar in that mineral. On minuter inspection, however, it is nothing but limestone. Its softness, its strong effervescence in acid, and specific gravity, (2.67) separate it from every substance with which it might be confounded. Besides the form I have mentioned, it sometimes becomes altogether earthy, and then reminds us of the most common form of deposits from calcareous springs : at other times it is altogether crystalline, and then passes into a fibrous form, resembling satin-spar, or calcareous alabaster. It has been deposited at the side of a *couleé* of basalt, and it is here that Capt. SLEEMAN discovered the remains of palm trees changed to a brown-coloured flint, or rather jasper. As one kind of palm tree (the date palm, I believe) yet commonly grows by the side of most running streams in this part of the country, we have no reason to suppose any change of climate to account for their appearance here. The manner, however, in which they are scattered through the soil is not so easily explained. They are usually found above the solid stratum of calc-tuff, and a trunk is seldom found entire ; but they are in sharp angular* fragments, as if they had been shattered by a violent blow : with them are pieces of the calc-tuff, which is found below. In the short distance from *Patteria* to *Ságar* we had met with two of these for-

* Nor do we find traces of any such substances disseminated through the tufaceous limestones, as is commonly the case in volcanic tuff.

mations*. They are nothing but kankar somewhat more developed, and probably were deposited at a period when the continent was raised above the level of the surrounding ocean. Among the remains, however, from the neighbourhood of *Jabalpur*, which appear also to have come from a recent calcareous deposit, are shells which appear to be marine. At *Tuismahl*, about 30 miles north-east from *Ságar*, I had an opportunity of observing another mineral more largely developed than I had seen it in the country to the eastward. This is the hydrated iron ore, which occurs in loose pieces about *Bardwán* and *Bankúra*, often accompanied by kankar. It is, I believe, the laterite of Dr. Buchanan, and here forms the summit of the *Tuismahl* hill in a bed of many feet in thickness. For the reason why a deposit from springs can thus cap an isolated hill rising out of a plain, I must refer to M. Montlosier's ingenious explanation of the isolated peaks and platforms of basalt in Auvergne. This mineral is largely developed in the country to the north of *Tuismahl*, and is, I believe, the ore which is usually smelted for iron.

We left *Tuismahl* in a N. W. direction, and soon came upon the sandstone again. It is, to be sure, occasionally to be seen in isolated ridges rising out of the basalt; but now this latter disappears, and it becomes the formation of the whole country round us. We find the basalt again some miles before reaching *Isságarh*, a fortress about 50 miles north of *Seronj*, and it here shews more symptoms of a recent formation than I have yet seen. The *couleés* are better defined; they have evidently, in some places, taken the lowest ground, and their surface is yet rugged in a degree, but their composition is, as before, a solid basalt. We quit the basalt altogether at *Isságarh*, and come upon the sandstone, which we travel upon to *Pahárgarh*, about 30 miles west of *Gwalior*, where we descend into the plain, and find ourselves again among kankar banks and ravines. The sandstone remains unaltered in character. In the bed of the Betwa it was quartz rock. In the country round *Delhi* it is usually quartz rock, nearly perpendicular, and dipping to the eastward. A few miles to the south of *Pahargarh* I observed a peculiar appearance of the kankar. It forms a calcareous cement to a bed of rounded pebbles, and above this forms another bed similar to those which are to be seen so frequently on the banks of the Ganges.

* There is no known force but that of an earthquake that could produce such effects. From Dr. Spilsbury's account of the fossil shells he found near *Jabalpúr*, they appear to be scattered through the soil in a similar manner.

IV.—*On the Native Alum or Salájít of Nepal. By* A. Campbell, *Assistant Surgeon, &c.*

In the number of the Asiatic Society's Journal for June last, there is a notice and analysis of one of the mineral productions of *Nepal* called "Salájít," or, by the natives of this place more commonly "*Pathar ka Passeo*" or simply "*Passeo*" (Sweat). As the analysis was furnished by Mr. Stevenson with the object of bringing the substance to public notice towards its extensive employment in the arts, the following particulars regarding it may I hope contribute in some degree to facilitate the above purpose. The specimen analyzed by Mr. S. contained in 100 parts, 95 parts of sulphate of alumina, but it is not generally speaking procurable in that state of purity; the following, the result of examination by Captain Robinson of several portions taken at random from the bazar, shews more correctly the value of the mineral as it is obtainable in large quantity, and in the state in which alone it could be made available for use in the arts. The purer portions being in such demand in medicine and surgery, are raised in price to an extent quite incompatible with their profitable application to the general uses of commerce. In 100 grains are contained, sulphate of alumina, 66.

The mineral in the above state (often more pure) is found throughout the lower, central, and upper hills of *Nepal.* Its external characters are those described by Mr. S.* save that the lumps have generally an admixture of red sand, and frequently portions of micaceous stone embedded in them; some of the lumps have the smooth surface of stalactites, and are not unlike these deposits. All are readily soluble in water, and when touched with the tongue give the taste of common alum. It is said to exude in this state from the surface of soft rocks; and sometimes to be dug out of their substance; and from these sources it is collected in considerable quantities during the cold and dry seasons, and carried by the Bhoteahs, Múrmis, and other hill people to *Katmandú,* to be exchanged with the merchants of that city for money or other articles. From hence it is distributed thoughout the valley in small portions for medical purposes, while the bulk of it is carried to the plains of India by petty Newâr merchants, and the numerous Baiparís who annually visit this country from various adjacent, and remote parts of Hindústan. The cost of production and transport of an article to the scene of its consumption, is the first knowledge the trader wants; and if the price paid by Mr. S. for his specimen (one rupee for two rupees' weight) was the real value of Salájít on the banks of the Ganges, its use in the arts

* In small light lumps, colour brownish white; externally anhydrous; internally semi-crystalline; fracture slightly fibrous, with a lustre resembling asbestus; porous, containing small cavities lined with scarcely perceptible needle-like crystals; adheres a little to the tongue. Taste acidulous saline, soluble in twice its weight of distilled water; specific gravity not ascertained, but probably not quite double the weight of distilled water; friable.

of dyeing, printing, &c. at that place, as at any other further removed, must for ever remain problematical. The price he paid for it was that which the physicians of India give for a drug to which they attach an undue merit, and on the sale of which they realize a huge profit from their credulous and ignorant patients. A respectable authority tells me that he has paid for this stuff at Benares one rupee for one rupee weight, and at more remote places from *Nepal* it is sold at a rate still more exorbitant. The average price of white* *Salájít* in *Katmandú* ranges from 12 annas to one rupee a *dharné* of three cacha seer, or from 11 to 15 rupees per pakka maund of 40 seers, and the cost of transport to the banks of the Ganges or Gandack is as follows:—

A hill porter will carry two maunds from hence to *Hitounda* for two rupees one anna, and a bullock will carry from thence to *Patna* four maunds at a charge of two rupees seven annas, or from the same place to *Govindganj* (on the *Gandak*, 10 miles south of *Bettiah*) for one rupee 14 annas. Thus the mineral can be stored at *Patna* at an average cost of from 14 8 to 18 8 rupees per maund, and at *Govindganj* for 15 annas per maund less, i. e. for 13 9 to 17 9 rupees. This calculation except the carriage from *Hitounda* is made in Nepalese rupees, the difference between which and sicca rupees is as 128 of the former to 100 of the latter, and there is no additional expense except an export duty of $2\frac{1}{2}$ per cent. *ad valorem*, levied by the *Nepal* government, unless there be (unknown to me) an import duty levied in our provinces, on minerals the product of this state.

The quantity now annually exported from *Katmandú*, as far as I can ascertain, is not more than 15 or 20 maunds, but I believe that there would be no difficulty in procuring any quantity required of it, and that without any addition to the present cost; for as it is found without the previous expense of digging mines, and transported without the necessity of making roads, an increased demand would only have the effect of inducing a greater number of the hill people to collect the stuff in the hills of their neighbourhood, and convey it to the capital; or perhaps with a steady demand the produce of the lower hills would be carried direct to the plains by the collectors of it, and the profit of the first buyer or *Katmandú* merchants by this means saved to the consumers in the plains. *Salájít* in Nepal as well as in India is at present confined exclusively to use in medicine and surgery, and in both countries it enjoys a very high reputation, and is used in both as a remedy in the same diseases.

In India it is in much greater repute than in the land of its production, as its price there shews; and its virtues in some affections are

* There is a dark bituminous substance used in *Nepal*, said to be exuded from rocks; it is called "Black Salájit." I am ignorant of its nature; it resembles in external character the bituminous alum ore (called shale) which is said to be found in Sweden and in many coal mines in England, but there is much vegetable matter in it, and it is probably a vegetable production, notwithstanding the belief by the *Nepal* physicians of its mineral nature.

said to be unequalled. Internally it is given as a sovereign remedy in *parméo* (gonorrhœa), in gleet, gravel, stone in the bladder, seminal weakness, and sometimes in alvine fluxes; its dose is (to an adult) 10 grains finely powdered and given in ghee: it also composes an ingredient in several of the compound medicines administered by the native physicians, and is said (possibly with justice) to be an admirable remedy in gravel as well as in diarrhœa. Externally it is chiefly employed in powder as a styptic in recent wounds, and, in solution, to bruises and sprains, as well as a wash for foul ulcers. In severe cases of falls and bruises it is internally administered, apparently without any better reason than the one stated by themselves, viz. that its being good for a bruised leg "ought to make it useful to the internals of a hurt man." It is much prized by old women as a remedy in infantile diseases, such as slight fever, diarrhœa and bronchitis—and few faqírs who dispense health to the body are without this mineral. It is exported, from *Nepal* in small quantities to almost every part of India, as few traders, from the horse merchant of the *Panjáb* to the Baipárí of *Tirhoot,* leave this without some of the drug, and the faqírs, who flock here annually in incredible numbers, distribute their small stores to their brethren of the craft at every pilgrimage from *Jagarnáth* to *Mánsurwar*, and from *Rámeswar* to *Dwaríka.* This mineral is not confined to *Nepal;* it is a produce of some part of Behár*, and is said to be found in small quantity in different parts of the Vindhya range of hills; although according to the testimony of the *Katmandú* merchants "of inferior value in medicine to that of *Nepal.*" Its use in the arts of calico printing, dyeing, &c. does not seem to have been contemplated even in India, where those arts have been so long practised; and although printing is done after a rude fashion throughout the valley of *Nepal,* and the mineral is a native of its surrounding hills, I cannot learn that it is ever used in the making of mordants, for which purpose the sulphate of alumina is above all other salts the best adapted, and for which it is in such large demand throughout Europe. It remains therefore for European intelligence to introduce this mineral into general use, and when it is considered that all the alum used in Europe for dyeing, printing, whitening paper, tanning and dressing leather, &c. &c. is manufactured by a tedious and expensive process, it will seem strange that a nearly pure native sulphate of alumina should be so abundant within a few days' journey of the river Ganges, and not have long ago, attracted the attention of the mercantile community of India, or the numberless dyers, printers, and tanners carrying on their separate vocations throughout the Gangetic valley. To assist Mr. Stevenson or any other person in procuring this substance, I offer such aid as being on the spot will enable me to give.

* Dr. Hamilton in his account of *Nepal* says, "I have collected Salájít in Behar with my own hands."

V.—*Defence of Lt. Burt's Trisection Instrument.*

To the Editor of the Journal of the Asiatic Society.

Sir,

The accompanying observations regarding the correctness of Mr. Burt's instrument for trisecting angles, described in No. 11, suggested themselves to me, in consequence of my attention having been drawn to it by some remarks contained in a note at page 159 of No. 15, and I take the liberty of sending them to you, in the hope, that should you think them likely to be of interest to any of your readers, you will give them a place in the Journal of the Asiatic Society. As it is not improbable, however, that ere this letter reach you the subject may have been taken up by a more able correspondent, or that its object may have been anticipated by Mr. Burt himself having forwarded a reply in defence of his invention, I hope that in either of these cases you will have no hesitation whatever in laying this communication aside.

In the note above alluded to, it is objected to Mr. Burt's demonstration of the correctness of his instrument, that the rad. $b\ o$ is not proved equal to rad. $a\ o$, and that it is in consequence imperfect. In the way however in which I understood the description, it appeared to me that the length of $o\ b$ was *constant*, the leg $a\ d$ being confined to a fixed point of it by a groove; and although not so expressed, I imagine it must have been intended that, that point should be at an equal distance from the centre with the point a. Should this supposition be correct, the demonstration would, I imagine, be complete, without the necessity of proving that the locus of the point b is the circumference of the circle ; but that such is the case whenever the angle is trisected, would be easily demonstrated as follows :—Let BAF (*fig.* 1) be any angle whereof BF is the chord, and let AC be the line trisecting the angle BAF and crossing the chord BF in D. It is required to prove that if from the point B with the radius BD an arc be described cutting AC in C (whence BC=BD) then, that the point C shall be situated in the circumference of the circle whose centre is at A and radius=AB, or that AC will equal AB.

First $\angle$ BDA $=\angle$ DAF $+\angle$ AFD (*El.* I. 32)
$=\angle$ DAF $+\angle$ ABD (*Hyps.*) (No. 1.)
Again $\angle$ BDA $=\angle$ DBC $+\angle$ BCD
$=\angle$ DBC $+\angle$ BDC
$=\angle$ DBC $+\angle$ DAB $+\angle$ ABD
Equating these two values of $\angle$ BDA, we have
$\angle$ DAF $+\angle$ ABD $=\angle$ DBC $+\angle$ DAB $+\angle$ ABD
Taking $\angle$ ABD from each side, $\angle$ DAF $=\angle$ DBC $+\angle$ DAB
But $\angle$ DAB $=\frac{1}{2}\angle$ DAF (by hyp.) therefore $\angle$ DBC also $=\frac{1}{2}\angle$ DAF
Whence also $\angle$ DAB $=\angle$ DBC
But the angle ADF or its equal $\angle$ BDC, or $\angle$ BCA
$=\angle$ DAB $+\angle$ ABD or
$=\angle$ DBC $+\angle$ ABD or
$=\angle$ ABC
Whence AB $=$ AC.

With regard to the latter part of Mr. B.'s paper, concerning the removal of the fourth leg of the instrument, I am not quite sure that I fully comprehend the mode in which the construction of the scale is detailed. If, however, the follow-

ing be a correct explanation of the meaning, there can, I imagine, be no hesitation in admitting the conclusion he has drawn. In forming the scale of equal parts upon this fourth leg AB (*fig.* 2) each point in the scale is to be successively brought to the circumference by turning the scale round the point A, so as that each division shall in turn terminate a chord of the variable arc AG, and the *line marking the division* is then to be cut on it, in the direction of the radius passing through it. At the same time the leg AD being placed in its corresponding position (viz. at an equal distance on the other side of a perpendicular to CG), its divisions will be marked by the *same* radius, and this is to be done for every point of the circumference AGgB'.

The divisions upon AD, therefore, form a scale of chords equal in length to those of the corresponding arcs AG, Ag, and each of the lines forming them, will by the construction tend to the centre when AD is so situated as to cut off an arc three times the extent of that of which AG is the chord ; and the application of the instrument merely consists in adjusting the line AD to the chord of the given arc, and then turning round the movable radius CG till it coincide with the division, which in that position would if produced pass through the centre, and which, if the coincidence be exact, will of course direct the radius to an arc one-third of AD. It must however be shown that in any position of AD there can be *only one* of the divisions which tends to the centre (or can be made to coincide with the radius), this may be easily proved; for if FL (*fig.* 2) be the correct division on the scale, cutting off (by radius passing through it) the arc AG = one-third of the arc AD, and if fl be any other division belonging to an arc A g the whole of the divisions having been marked off in the manner above described, then it may be demonstrated that the radius Cf will, if drawn through f, form with fl an angle lfn equal to the angle fCF plus half the angle gCF*. The instrument therefore seems to be complete enough in theory without the fourth leg, but in use, it appears to me that the want of it would considerably diminish its accuracy, as it must be very difficult to hit upon the exact coincidence when the divisions are very numerous, and as any error at the point F would be multiplied at G in the proportion of the two distances CF : CG, this would be a serious evil in large angles, as the focus of the point F is a curve which

* The demonstration of this is as follows, vide fig. 2.

$$\begin{aligned}\text{First } \angle\, CGA &= \angle\, GCM + \angle\, CMG\\ &= \angle\, GCM + \angle\, CgA - \angle\, gAM\\ &= \angle\, GCM + \angle\, CgA - \tfrac{1}{2}\angle\, GCM \ (\textit{El.}\ \text{III. 20.})\\ &= \angle\, CgA + \angle\, \tfrac{1}{2}GCM\end{aligned}$$

$$\left.\begin{aligned}\text{But } \angle\, CGA &= \angle\, AFL\\ \text{And } \angle\, CgA &= \angle\, AFL\end{aligned}\right\}\ \text{(by hypothesis)}$$

Therefore substituting these values in above equation

$$\angle\, AFL = \angle\, Afl + \tfrac{1}{2}\angle\, GCM \ \text{(No. I.)}$$

Again $\angle\, CfD$ or $Afn = \angle\, CFf + \angle\, fCF$

Or $\angle\, AFl + \angle\, nfl = \angle\, AFL + \angle\, fCF$

Or, by substituting the value of $\angle$ AFL found at (No I.)

$$= \angle\, Afl + \angle\, fCF + \tfrac{1}{2}\angle\, GCM$$

And subtracting $\angle\, Afl$ from each side of the equation

$$\angle\, nfl = \angle\, fCF + \tfrac{1}{2}\angle\, GCM$$

Q.E.D.

gradually approaches, and ultimately (when the angle trisected equals 180°) passes through the centre.

Mr. B. says that the fourth leg is absolutely necessary to the first construction of the instrument, but it has occurred to me that by forming the scale upon AD in a different manner it might be dispensed with altogether. For since AG is always equal to AF, and consequently the angles AFG and AGF also equal, it follows that if the arm AB be turned round till AG coincides with AF, that the point G will also coincide with the point F, and the line FL would form an angle with AG; as for instance the angle AGp equal to the angle AFL or AGF. The instrument would therefore I think be equally correct if the divisions upon AB were first drawn in the way that Mr. B proposes, as above described, and, if this leg when complete were afterwards converted into the chord AD by *reversing* the inclinations of all the lines Gq, making them form equal angles on the opposite side of a perpendicular to AB, for then $\angle p$GA would be equal $\angle$BGq=$\angle$ FGA=$\angle$ AFG.

As I before observed, the locus of the point F (*fig.* 2) is a curve passing through the centre C. A representation of this is given in fig. 3, which also shows it continued, and passing through the extremity of a diameter at right angles to GC, which it again meets at M, GM being equal to GL, the diameter of the circle GDL. From the circumstances of the distance DK being always equal to 2 vers-sin. $\angle$ DCG (which may be easily deduced from Mr. Burt's theorem) may be derived an equation to the curve when the co-ordinates originate at the centre (r being =GC)

$$y^2 = 2rx - x^2 + \frac{r^2}{2} - r\sqrt{2rx + \frac{r^2}{4}}\ *.$$

As it is also easily described geometrically, it affords a very simple form for the construction of an instrument for trisecting any angle from 0 to 180°, and consisting of a *single* piece only. A representation of one which I have lately made up, and found to answer my expectations fully, is given in fig. 4. It consists simply of an ivory scale, whose edge is sloped off, and accurately formed to the figure of the curve GKC (*fig.* 3), and a small part of the diameter GL produced on each side to ensure its accurate adjustment to one of the sides containing the given angle, for which purpose also small portions of the edge at C and G are cut away, in order that the coincidence of these two points with the centre and point G of the chord of the given angle may be accurately determined. As no graduation whatever is necessary the instrument is very easily made, and the application of it, which is also extremely simple, will be understood from the following example: I must first mention, however, that for more convenient measurement the exact length of the radius GC is laid off on the centre of the scale between the points M and N.

Let GCD (*fig.* 4) be any angle to be trisected.

From the point C with the distance CG or MN as a radius, describe an arc GLD. Draw the chord GD, then apply the scale so as to make its edge coincide with the side CG of the given angle, and the point C with the centre of the circle

* From this equation may be derived the other properties of the curve just mentioned. For instance if x be taken equal to o, then y becomes $= o$ or $r =$ CH. If $x = r$, then y also becomes $= o$ or $r\sqrt{3} =$ GF; and lastly, if x be taken equal to 3 r, then y becomes $= o$ or an imaginary quantity. The curve will therefore pass through the points G,C,H and M.

GLD, and of course since the radius is by construction equal to GC, the point G will coincide with the point G (of the chord). Make a mark at the intersection of the curve GKC with the chord GD, and a line drawn from the centre through that point (K) will trisect the given angle. As the curve GKC is the locus of the point D in fig. 1, when DB is equal to BC and AB=AC, which corresponds with the conditions of Mr. Burt's demonstration, it is unnecessary for me to trouble you with any proof of the correctness of the instrument in addition to that already given by him. By extending the principle and making use of different curves† and with some necessary modifications, an instrument might be constructed, in a *single piece*, to divide any given angle in *any* given ratio (within moderate limits), but as I have no hopes that any contrivance for this purpose, however simple in application, or comprehensive in its powers, will ever supersede the good old method by trials with the compasses, I shall not further trespass on your patience by indulging in any useless speculations on the subject.

I am, Sir,

Your obedient servant,

J. S.

Masulipatam,
3rd July, 1833.

P. S. I imagine Mr. Burt's parallel lines passing through the same point are intended to be referred to different places, being coincident, and passing through the centre when referred to the plane of the instrument, but parallel when referred to one perpendicular to it.

VI.—*Computation of the Area of the Kingdoms and Principalities of India.*

Captain J. Sutherland, late Private Secretary to the Vice-President, having been recently engaged in the preparation of a note on the political relations of the British Government in India, adopted a mode, on the recommendation of the Surveyor General, of obtaining in a rough way the area, or contents in square miles, of each state, without the labour of elaborate calculation, to which the imperfect data of our maps of the country could not ensure very great accuracy.

The boundaries of each state having been marked off on a skeleton map drawn on paper, of equable texture, as accurately as this could be done from information procurable in the Surveyor General's Office and the Political Department, the whole were cut out with the greatest care, and weighed individually, and collectively as a check, in the most delicate balance of the Calcutta Assay Office. The weights were noted to the thousandth part of a grain, the balance being sensible to the tenth part of that minute quantity. Fifteen precisely equal squares of paper (unfortunately

* If the *whole* curve be used and the chord produced each way, it will intersect it in three points (as shown at KK'K" *fig.* 3), giving as many positions of the line CK, and as many solutions of the problem. In this case the intersection with the interior loop, as at K, marks the third of the given angle, while those with the exterior branches of the curve trisect its complement (or if larger, its excess beyond 90°) and supplement.

† The locus of the point of bisection for instance would be a semicircle on the diameter GC.

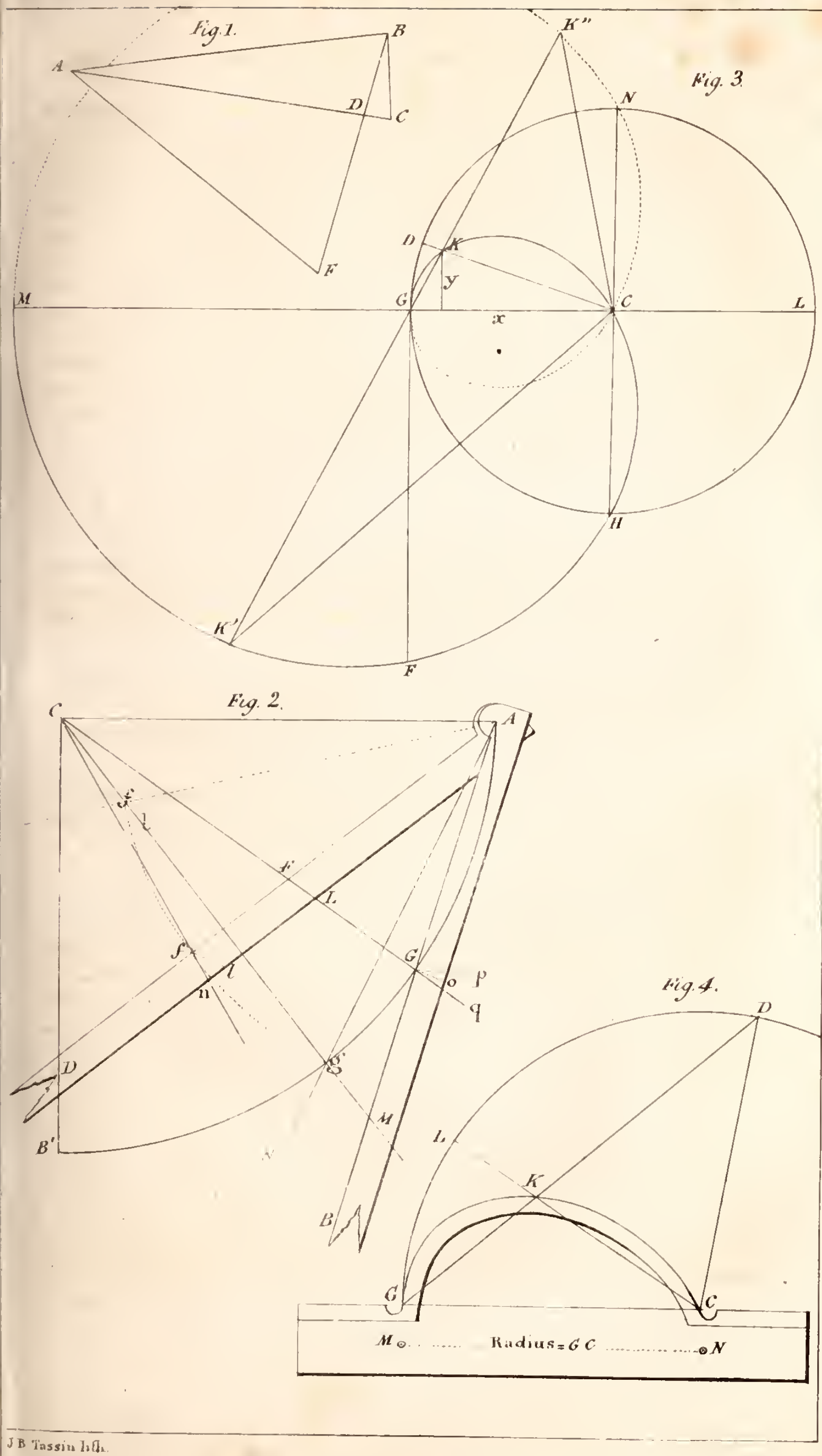


Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

not the same as that used for the map) were previously weighed to ascertain the extent of variation to which such a mode of measurement would be liable: the results were not very favorable, neither was the paper of such equal texture as might be fairly compared with that used for the map: the weights were as follows—(to the nearest hundredth of a grain,) apparently increasing towards the edges of the sheet.

1 = 2.65 grains,	6 = 2.95 grains,	11 = 3.05 grains.
2 = 2.65	7 = 2.90	12 = 2.75
3 = 2.65	8 = 2.90	13 = 2.65
4 = 2.68	9 = 2.80	14 = 2.65
5 = 2.80	10 = 3.10	15 = 2.75

Before setting to work on the states, an index or unit of 100 square degrees, cut from the same paper, was first weighed to serve as a divisor for the rest.

The weighing process commenced in the driest part of the day, taking the whole of the papers together; thus the continent of India weighed 127.667 grains troy. The sum of the individual weights of the separate states was 127.773. The addition was proved to proceed from the hygrometric water absorbed towards the evening; thus weighed, the British states weighed at first 74.366, at the conclusion 74.445; the native powers, at first 53.301; the sum of them weighed individually was 53.407; afterwards, weighed in groups 53,456, being later in the evening. In drawing out the table for calculation, proper corrections were applied to neutralize this source of error, but coupled with the previous examination of the texture of paper, it is sufficient to shew that the following table must be looked upon only as a rough approximation in the absence of better information. The superficial area of Hindustan, exclusive of the independent states of Nipal, Lahore, &c. according to Hamilton, between the latitudes of 8° and 35° north, and the longitude of 68° and 92° east, cannot be estimated at more than 1,280,000 English square miles: and the portion belonging to the British and their allies, at 1,103,000: this estimate agrees very well with the present statement.

	Square miles.
The area of the native states in alliance with the British Government was found to be,	449,845
That of the territory under British rule with the remaining small states and jágírdars,	626,746
Superficial area of all India,	1,076,591

The extent of coast from Cape Negrais to the frontiers of Sinde is 3622 British miles, the breadth from Surat to Silhet, 1260 miles.

Captain Sutherland classifies the native states of India under the three following heads:

I.—*Foreign*, viz. Persia, Kabul, Senna, the Arab tribes, Siam, Acheen.

II.—*Externat, on the frontier;* viz. Ava, Nepal, Lahore, Sinde.

III.—*Internat,* which are those included in the present list. All of these have relinquished political relations with one another and with all other states. They are, according to the nature of their relations or treaties with the English, divided into six classes:

First Class. *Treaties offensive and defensive: right on their part to claim protection, external and internal, from the British Government: right on its part to interfere in their internal affairs.*

		Square miles.	Square miles.
1.	Oude, containing, by weighment,....	23,923	by HAMILTON*, 20,000
2.	Mysore, ...	27,999	 27,000
3.	Berar or Nagpúr,................	56,723	 70,000
4.	Travancore, ...	4,574	 6,000
5.	Cochin,..................	1,988	 2,000

SECOND CLASS. *Treaties offensive and defensive: right on their part to claim protection, external and internal, from the British Government, and to the aid of its troops to realize their just claims from their own subjects: no right on its part to interfere in their internal affairs.*

		Square miles.	Square miles.
6.	Hyderabad, containing, by weighment,	88,884	by HAMILTON, 96,000
7.	Baroda,........................	24,950	 12,000

THIRD CLASS. *Treaties offensive and defensive, states mostly tributary, acknowledging the supremacy of, and promising subordinate co-operation to, the British Government; but supreme rulers in their own domains.*

8. Indore, containing,................ 4,245 square miles.

Rajputána States.		Square miles.			Square miles.
9.	Oudípúr, (H. 7,300,)	11,784	16.	Jesalmír, ...	9,779
10.	Jeypúr, ...	13,427	17.	Kishengurh, ...	724
11.	Joudpúr,..	34,132	18.	Banswára, ...	1,440
12.	Kotah, (H. 6,500,)	4,389	19.	Pertábgurh, ...	1,457
13.	Búndí, (H. 2,500,)	2,291	20.	Dúngarpúr, ...	2,005
14.	Alwar,............	3,235	21.	Kerolí, ...	1,878
15.	Bikhanír,........	18,060	22.	Serowí, ...	3,024

			Square miles.
	23.	Bhurtpúr, (by HAMILTON, 5,000,)............	1,946
	24.	Bhopal, (ditto 5,000,)......................	6,772
	25.	Kutch, (H. with the Runn 13,300,)............	7,396
	26.	Dhár and Dewas, ...	1,466
	27.	Dhólpúr, ...	1,626
Boghelkhand, and *Bundelkhand*,	28.	Rewah, ...	10,310
	29.	Dhattea, Jhánsí, Terhí, ...	16,173
	30.	Sawantwarí, ...	935

FOURTH CLASS. *Guarantee and protection, subordinate co-operation, but supremacy in their own territory.*

31.	Ameer Khan,	Tonk,......	1,103	.... 1,633 square miles.
		Seronj, ...	261	
		Nimbahara,	269	

32. Patiala, Keytal, Naba and Jeend, ... 16,602

FIFTH CLASS. *Amity and friendship.*

33. Gwalior, containing,..................... 32,944 square miles.

SIXTH CLASS. *Protection, with right on the part of the British Government to control internal affairs.*

34. Sattara, containing, ... 7,943 square miles.

35. Kolapúr ,............................ 3,184

* This column, and other items marked H., extracted from HAMILTON's Hindustan by way of comparison.

Of the above states, four are Mohammedan; viz. Hyderabad, Oude, Bhopal, and Tonk. Of the Hindu states, eight are Marhatta; viz. Sattara, Gwalior, Nagpúr, Indore, Banda, Kolapúr, Dhar, and Dewas.

Nineteen are Rajput; viz. Oudípúr, Jeypúr, Joudpúr, Búndí, Kotah, Kutch, Alwar, Bhikanír, Jesalmír, Kishengarh, Bánswára, Pertábgarh, Dúngerpúr, Kerolí, Serowí, Rewah, Dhattea, Jhansí, Terhí.

Six are of other Hindu tribes; viz. Mysore, Bhartpúr, Travancore, Sáwantwárí, Cochin, and Dholpúr.

Besides these allied states, there are the following inferior Rajships and Jágírdarís: viz. Chota Nagpúr, Sirgújer, Sambhalpúr, Singhbhum, Oudípúr, Manipúr, Tanjore, the Bareich family, Ferozpúr, Merich, Tansgaon, Nepaní, Akulkote, and those of the Ságar and Nerbudda country; also Sikkim and the states of the northern hills.

VII.—Miscellaneous.

1.—*Importation of Ice from Boston.*

The arrival of the Tuscany, with a cargo of ice from America, forms an epoch in the history of Calcutta worthy of commemoration, as a facetious friend remarked, in a medal of *frosted* silver. In the month of May last, we received a present of some ice from Dr. Wise at Huglí, (whose efforts have so long been directed to the extension of its manufacture by the native process*,) as a proof that the precious luxury might be preserved by careful husbandry until the season when its coolness was most grateful:—little did we then contemplate being able to return the compliment with a solid lump of the clearest crystal ice, at the conclusion of the rains! nor that we should be finally indebted to American enterprise for the realization of a pleasure for which we have so long envied our more fortunate country-men in the upper provinces; nay even the beggars of Bokhara, who, in a climate at times more sultry than ours, according to Lieut. Burnes, "purchase ice for their water even while entreating the bounty of the passenger †!" Professor Leslie, with his thousand glass exhausters, and his beautiful steam air-pumps, tantalized us with the hopes of a costly treat, and ruined poor Taylor the bold adopter of his theory:—but science must in this new instance, as on so many former occasions, confess herself vanquished or forestalled by the simple practical discovery that a body of ice may be easily conveyed from one side of the globe to the other, crossing the line twice, with a very moderate loss from liquefaction.

We are indebted to Mr. J. J. Dixwell, the agent for the proprietors, for the following interesting particulars relative to the Tuscany's novel cargo, and the mode of shipping ice from America for foreign consumption.

The supplying of ice to the West Indies and to the Southern States of the Union, New Orleans, &c. has become within these few years, an extensive branch of trade, under the successful exertions of its originator Frederic Tudor, Esq. of Boston, with whom S. Austin, Esq. and Mr. W. C. Rogers are associated in the present speculation.

* See page 80 of the present volume, and former notices in the Gleanings.

† Journal, vol. ii. p. 229.

The ponds from which the Boston ice is cut are situated within ten miles of the city. It is also procured from the Kennebec and Penobscot rivers in the State of Maine, where it is deposited in ice houses upon the banks, and shipped from thence to the Capital. A peculiar machine is used to cut it from the ponds in blocks of two feet square, and from one foot to eighteen inches thick, varying according to the intensity of the season. If the winter does not prove severe enough to freeze the water to a convenient thickness, the square slabs are laid again over the sheet ice, until consolidated, and so recut. The ice is stored in ware-houses constructed for the purpose at Boston.

In shipping it to the West Indies, a voyage of 10 or 15 days, little precaution is used. The whole hold of the vessel is filled with it, having a lining of tan about four inches thick upon the bottom and sides of the hold, and the top lifts covered with a layer of hay. The hatches are then closed, and are not allowed to be opened till the ice is ready to be discharged. It is usually measured for shipping, and each cord reckoned at three tons: a cubic foot weighs 58½lbs.

For the voyage to India, a much longer one than had been hitherto attempted, some additional precautions were deemed necessary for the preservation of the ice.

The ice-hold was an insulated house extending from the after part of the forward hatch to the forward part of the after hatch, about 50 feet in length. It was constructed as follows:

A floor of one-inch deal planks was first laid down upon the dunnage at the bottom of the vessel: over this was strewed a layer one foot thick of tan, that is, the refuse bark from the tanners' pits, thoroughly dried, which is found to be a very good and cheap non-conductor; over this was laid another deal planking, and the four sides of the ice-hold were built up in exactly the same manner, insulated from the sides of the vessel. The pump, well, and main mast were boxed round in the same manner.

The cubes of ice were then packed or built together so close as to leave no space between them, and to make the whole one solid mass: about 180 tons were thus stowed. On the top was pressed down closely a foot of hay, and the whole was shut up from access of air, with a deal planking one inch thick, nailed upon the lower surface of the lower deck timbers; the space between the planks and the deck being stuffed with tan.

On the surface of the ice, at two places, was introduced a kind of float, having a guage rod passing through a stuffing box in the cover, the object of which was to note the gradual decrease of the ice as it melted and subsided bodily.

The ice was shipped on the 6th and 7th of May, 1833, and discharged in Calcutta, on the 13th, 14th, 15th, and 16th September, making the voyage in four months and seven days.

The amount of wastage could not be exactly ascertained from the sinking of the guages, because on opening the chamber it was found that the ice had melted between each block, and not from the exterior only in the manner of one solid mass as was anticipated. Calculating from the rods and from the diminished draught of the ship, Mr. DIXWELL estimated the loss on arrival at Diamond Harbour to be fifty-five tons. Six or eight tons more were lost during the passage up the river, and probably twenty in landing. About one hundred tons, say three thousand maunds, were finally deposited in the ice house on shore, a lower room in a house at Brightman's ghaut, rapidly floored and lined with planks for the occasion.

The sale has not, we believe, been so rapid as might have been expected, amounting to no more than ten maunds per diem, although Mr. Rogers has fixed the price at the low rate of 4 annas per seer, one half the price estimated for the Huglí ice, which was still calculated to be somewhat cheaper in proportion than saltpetre. The puhlic requires to be habituated to it, and to be satisfied of the economy of its substitution for the long established process of cooling. There may also be some douhts of the best mode of preserving so fleeting a commodity, but on this head we cannot hut advise an imitation of the methods pursued on a large scale on board of the Tuscany. For the application of the ice to the purposes of cooling ample directions have heen given in the Gleanings of Science, vol. iii. p. 120. A hox, or basket, or tin case, with several folds of blankets, or having a douhle case lined with paddy chaff or any non-conducting substance, will preserve the ice until wanted, and for cooling water or wine the most effectual method of all is to put a lump of the clear crystal into the liquid : the next best is to spread fragments upon the bottles laid horizontally, and leave them wrapped in flannel for a couple of hours.

So effectual was the non-conducting power of the ice-house on board, that a thermometer placed on it did not differ perceptibly from one in the cabin. From the temperature of the water pumped out, and that of the air in the run of the vessel, Mr. Dixwell ascertained that the temperature of the hold was not sensihly affected hy the ice. Upon leaving the tropic and running rapidly into the higher latitudes, it retained its heat for some time, but after heing several weeks in high latitudes, and becoming cooled to the temperature of the external air and sea, it took more than ten days in the tropics before the hold was heated again to the tropical standard.

Mr. Dixwell has favored us with a sight of the daily register kept hy himself on hoard, which we regret we have not space to insert at length :—The following extracts however will serve to impart some of the useful information gleaned in this first experimental passage from Boston : we sincerely hope and believe that it will afford ample encouragement for a repetition of the speculation, and eventually for a regular annual consignment of this new staple produce of the northern continents ! a scheme is now in circulation for supplying ice all the year round at 2 annas per seer.

Extracts from the Log of the Ship Tuscany.

Date. 1833.	Latitude at noon.	Longitude at noon.	Temp. of air.	Temp. of water.	Fall of Ice-guage.	Wind.	Remarks.
	° ′	° ′	°	°	inches.		
May 13	41 30 N	66 44 W.	61	—		S. W.	moderate.
16	39 26	54 51	64	70		N. W.	fresh.
20	34 24	43 00	70	68		W.	ditto.
24	27 48	33 44	71	73		N. E.	av. temp. in N. temperate zone.
28	20 48	30 29	78	76		E.	air 68.3—water 72.3.
June 1	13 05	25 33	80	79	4	N. E.	light airs.
6	7 29	19 51	86	85		N. E.	ditto.
16	0 52	18 05	84	83	7.1	S. E.	average temp. of N. tropic. A. 82° W. 81°.5.
19	4 04 S	22 21	82	80		S. E.	

[Having occasion to open the run scuttle, found the air pouring up from the hold quite warm : a therm. which stood at 80° in the cabin, rose to 84° in the run.]

30	23 29	28 29	76	73	9.2	E.	av. temp. S. tropic. A. 79°.9 W. 79°.0.

[July 2, bored with an auger into the ice house, under the main hatch, and came to ice at 10 inches from the top. Cargo as usual dry.]

July 4	29 38	19 38	72	70	[illegible]	W.	fresh winds.

[Instead of the usual 10 minutes pumping, required an hour to free the ship from water owing to cranckness, and having been 20 days on one tack. She has been pumped out generally 4 or 5 times, or about 170 strokes per diem.]

15	38 53 S.	12 27 E	51°	56°	guage.	S. W.	[current
18	39 22	23 10	68	70	11.5	N. W.	in the warm Cape

[Since leaving the trades, the water pumped from the hold has been 2° to 5° warmer than the sea.]

30	39 33	63 00	61	60	13.2	W.N.W	moderate.		
Augt. 6	28 35	83 01	62	65	14.3	S.	ditto.		
17	5 18	85 45	86	86	24.0	calm.	in cabin	83° in run	78°.5
19	4 36	85 28	89	86	24.0	do.		83	81
21	2 34	85 14	89	86	25.0	do.		84	82
23	0 59	84 57	89	86	25.0	do.		83	84
29	11 23N.	85 34	88	84	26.5	S. W.	fresh.		
Sept. 1	20 14	87 08	86	86		S. W.	ditto.		
3	Diamond	Harbour.	88	88		S. W.	light.		

Average temperature of S. Tropic Indian Ocean,		Air 81°.33	Sea, 81°.39
N. Tropic to Diamond Hr.		86.4	85.35
Average temperature of the whole voyage,	Air	73.89	Sea, 74.17
Do of former voyages—			
	6 June to 18 Sept. 1827	71.71	
	16 June to 6 Oct. 1828	71.28	
	15 June to 18 Oct. 1829	70.16	
	2 Aug. to 18 Dec. 1830	71.52	
	22 Aug. to 16 Jan. 1831-2	74.57	

2.—*On the Action of various Lights upon the Retina. By Sir* D. Brewster.

When the eyes are exposed to strong lights, objects cannot be seen of their true colours, and even lights of ordinary intensity produce a decided deterioration in the tints of a fine picture. Hence it is that we see paintings to most advantage when we view them through two blackened tubes held close to the eye. By this means the colours are not only more brilliant, but faint lights are brought out which would otherwise have been overpowered by the action of lateral light upon the retina. If we turn a picture upside down, and look at it with the head inverted, a similar effect is produced, because the image is received upon a part of the retina which is not so frequently used ; and it is for the same reason that the colours of the sky and of the landscape near the horizon are so beautifully seen hy looking at them either hetween the legs, or heneath the arm with the head inverted.

It is well known that the human complexion is seen to greater advantage in candle, than in day-light, uuless the complexions are very ruddy. This arises from there being so much more red in candle, than in day-light. There are certain states indeed, of the atmosphere, when dark-blue clouds prevail, in which the ordinary complexion appears to great disadvantage ; and persons in variable health are often described as looking ill, when the change arises from the prevailing colour of the clouds.

When gas-lights were first introduced, it was a common complaint among those who frequented the theatre that they injured the personal appearance of the audience. This bad quality made them so unpopular, that a red colour was communicated to the light by inclosing it in a reddish coloured glass. The effect, however, arose from the great quantity of light which was used, and from its influence upon the retina ; and if the same intensity of light had been obtained either from oil or from candles, the same effect would have heen produced. Our eyes are now so much accustomed to the use of strong lights that the retina is not so easily reudered insensible to the red rays, and the blue colour of the light is no longer complained of. It is, however, still observed, hy those who have been for the first time exposed

to gas illumination, and the eyes of such persons must therefore serve an apprenticeship before they learn to see objects in their true colours.

The blue colour of gas-light was ascribed to the badness of the gas; and the apparent removal of this injurious quality has been attributed to its increased purity and to improved methods of burning it: but the truth is, that bad gas, or an imperfect combustion of good gas, produces a much redder light than good gas burnt in the best manner. The smoke which is produced in the former cases invariably reddens the flame, and its perfect removal causes the gas to approximate to the light of the sun, which is always bluer than that of the whitest flames from wax, oil, or tallow.

There is a very pretty experiment illustrative of some of the preceding observations, which is easily made. Place two candles at the distance of two or three feet from the eye, and about one foot from each other, and having closed one eye, fix the other intently upon either of the candles, as if it were examining with attention some point of the wick. The other candle will be seen by indirect vision, and after a little time, it becomes much brighter and bluer than the first, in consequence of the part of the retina on which its light falls being more susceptible than the more frequently used portion in the axis of the eye, upon which the light of the second is incident. The higher degree of excitation of the retina, produced by the candle seen indirectly, renders that portion of the membrane less sensible to the red rays; and if the excitation is continued, the image will become actually blue, and will be surrounded with a halo of yellow nebulous light. The blue image, indeed, will sometimes disappear, and leave nothing in its place but a nebulous hole.—*Phil. Mag. March*, 1833.

3.—*Substances contained in Opium*

M. Pelletier in an elaborate memoir on opium printed in the *Annales de Chimie*, mentions the following principles as contained in opium; viz. narcotine, morphia, meconic acid, meconine, narceine, caoutchouc, gum, bassorine, lignine, resin, brown acid, and extractive matter, fixed oil, and a volatile, but non-oleaginous principle, which rises in distillation with water.

Added to these substances, M. Beter announces (Journal de Pharmacie, April, 1832), another peculiar principle; it is bitter, crystallizable, forms salt with acids, especially with acetic acid, with which it gives crystals in the form of very white scales, and with sulphuric acid, white silky crystals; no name is given to this substance by its discoverer.

M. Robiquet, it also appears, has separated a new alkali from opium, which he calls paverin. Only a few details of its properties are yet given (Journal de Pharm. Nov. 1832). It differs very remarkably from other vegeto-alkalies in being soluble in water; saturates acids, is insoluble in potash, and contains much azote; it is very poisonous, and acts very particularly on the spinal marrow.—*Phil. Mag.*

4.—*Death of Captain J. D. Herbert.*

It is with feelings of sincere grief that we record the loss of our most worthy friend and late coadjutor, Captain J. D. Herbert, at Lukhnow on the 24th instant. He had been for some time suffering under the effects of the climate: a sudden determination of blood to the head was the immediate cause of the fatal event.

Meteorological Register, kept at the Assay Office, Calcutta, for the month of September, 1833.

Day of the month.	Barometer reduced to 32° Farh. At 4½ A.M.	At 10 A.M.	At 4 P.M.	At 10½ P.M.	Thermometer in the Air. Minimum at 4½ A.M.	At 10 A.M.	Max. by Reg. Ther.	At 4 P.M.	At 10 P.M.	Depression of moist-bulb Thermometer. At 4½ A.M.	At 10 A.M.	At 4 P.M.	At 10½ P.M.	Hair Hygrometer. At 10 A.M.	At 4 P.M.	Rain. Inches.	Wind. Morning.	Noon.	Evening.	Weather. Morning.	Noon.	Evening.
1	,478	,520	,430	,471	80,2	86,0	96,0	85,0	84,0	2,7	4,0	4,0	3,0	95	96	0,37	o.	S.	o.	cumuli.	cistr. rn.	close.
2	,444	,490	,400	,476	81,5	84,4	85,2	83,8	83,0	2,1	3,8	3,7	3,2	99	98	1,40	o.	S.	S.	settled	rain	all day.
3	,429	,477	,430	,499	82,4	85,2	88,8	86,1	82,4	3,4	3,4	3,5	2,1	96	97		s.	s. w.	N.	strat.	overcast.	cum. nimb.
4	,483	,576	,504	,582	79,2	84,3	90,2	86,4	82,0	1,2	3,2	3,9	3,8	97	95	0,60	s. e.	e.	S. E.	do	rain.	cumuli.
5	,570	,628	,521	,610	79,4	85,2	89,8	86,1	80,2	1,9	3,4	3,5	2,0	98	96	1,20	s. e.	e.	E.	rain.	cumuli.	quite clear.
6	,570	,645	,541	,639	80,0	84,5	90,1	85,7	81,9	1,3	3,4	3,7	3,3	98	97	0,20	e.	e.	E.	clear.	fair.	clear.
7	,600	,676	,587	,671	80,0	85,1	91,3	87,5	82,1	4,7	4,2	5,7	3,6	95	91	0,08	e.	E.	E.	stratus.	do	do
8	,624	,697	,614	,675	79,0	85,5	91,4	88,5	81,2	4,2	4,2	4,8	2,6	95	94		e.	s. w.	s.	cumuli.	stratus.	do
9	,664	,743	,627	,720	80,2	85,6	91,3	88,9	82,8	2,2	5,0	5,7	4,0	93	91	0,20	s.	s.	s. e.	clear.	fine.	do
10	,685	,761	,630	,730	81,0	85,8	96,0	90,	84,2	2,0	5,2	6,0	3,7	93	90		s.	S.	S.	do	do	do
11	,678	,763	,633	,741	82,2	87,2	94,0	90,4	84,2	2,2	4,0	5,5	4,0	95	90		s.	s.	S.	do	do	do
12	,712	,769	,662	,734	82,2	87,3	93,3	89,5	83,7	2,7	5,0	6,5	4,5	94	90		s.	s.	S.	do	clear.	do
13	,676	,750	,648	,677	81,8	86,5	92,8	90,0	83,8	6,4	5,3	7,2	4,8	93	89		S.	w.	S.	do	do	do
14	,647	,700	,570	,618	83,2	86,8	93,7	91,5	79,0	4,7	5,0	6,2	3,0	93	91		S.	s. w.	s,	do	do	rain th. l.
15	,604	,657	,564	,622	81,3	87,7	95,8	92,0	85,6	2,6	4,6	5,4	3,0	95	93		S. E.	s. w,	s, w.	do	do	hazy.
16	,615	,701	,600	,662	83,3	87,5	94,0	91,5	84,5	2,2	4,5	5,6	3,5	95	92		S.	S.	S.	dark.	do	clear.
17	,624	,702	,583	,649	83,1	87,5	93,6	91,1	82,2	2,4	5,0	7,5	2,4	95	88		s.	S.	S.	nimbi,	do	do
18	,622	,660	,558	,664	82,0	87,5	101,3	92,5	84,2	2,7	5,0	10,0	4,2	93	83		s.	s. w.	S.	clear.	do	do
19	,644	,688	,603	,695	82,4	88,2	93,2	89,5	83,2	2,6	5,4	6,3	3,5	92	99		o.	s. w.	s. e.	do	do	do
20	,674	,737	,601	,667	81,8	87,5	95,7	91,1	83,4	3,0	5,4	8,2	3,9	92	87		o.	s.	s.	do	cumuli.	bank s. w.
21	,625	,671	,547	,623	83,0	88,8	102,0	91,6	84,3	2,8	6,2	8,2	3,3	91	86		o.	n. w.	o.	do	fair l.	threatening.
22	,576	,622	,534	,619	82,6	87,6	93,2	85,2	82,0	2,5	4,5	4,8	2,0	96	95	0,82	n.	n. e.	s. e.	do	rain. th.	haze.
23	,590	,634	,542	,612	81,2	86,6	90,2	85,5	80,3	2,5	4,3	4,3	2,1	95	97	0,48	E.	n,	E.	nimbus.	cum.rn.th,	threatening.
24	,563	,652	,546	,634	80,0	85,4	89,9	86,7	80,0	1,3	4,2	4,5	2,0	96	96	0,10	E.	E.	E.	dark.	overcast. cir.	above-fixed.
25	,593	,662	,552	,632	79,1	85,4	89,0	85,0	82,6	2,0	3,7	3,8	4,0	96	97	0,60	n. e.	n. e.	S. E	do	do	cirri.
26	,606	,631	,538	,610	79,5	85,0	88,0	84,2	81,3	2,2	3,8	3,3	2,5	99	88	1,22	E.	s. e.	S. E.	cumulus.	rain.	cir. cum.
27	,574	,611	,498	,580	80,0	85,7	99,0	89,1	82,0	1,9	4,5	6,7	3,0	95	89		E.	S.	S. E.	clear.	clear.	very clear.
28	,524	,567	,460	,567	80,2	86,7	103,5	90,0	83,2	1,5	5,3	8,0	3,4	93	86		s. e.	E.	S. E.	do	do	cirri.
29	,522	,586	,436	,593	81,4	86,5	106,2	87,9	84,1	2,3	4,5	4,6	3,6	95	95		n. e.	E.	E.	do	cum.th.rn.	clear.
30	,570	,600	,500	,585	81,5	85,7	86,7	86,7	83,0	3,0	3,6	4,3	3,2	97	96	0,92	n. e.	n. e.	E.	nimbi.	cum. strat.	do.
Mean	29,593	,652	,548	,612	81,1	86,3	93,5	88,3	82,6	2,6	4,4	5,5	3,2	95	92	8,19	S. and E.			Squally much thunder.		

The Instruments for 10 A. M. and 4 P. M. are suspended in the free air of the Laboratory, the Barometer used at those hours stands ,044 lower than the Surveyor General's; the correction for the other Barometer will be given hereafter.